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GENERAL SECTION

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CONTENTS OF VOL. XVII

PART 1. JULY, 1926

PAGE

Regularity and Rhythmicalness. By H. E. O. JAMES . . 1

The Nature of Images. By MARGARET DRUMMOND . . 10

The Constancy of the Intelligence Quotient—Final Results. By P. L. GRAY and R. E. MARSDEN 20

A Comparison of Group Verbal and Pictorial Tests of Intelligence. By CONSTANCE M. DAVEY 27

A Note on the Effect of Polarisation in Psycho-Galvanic Experiments. By H. E. O. JAMES and R. H. THOULESS . 49

Psycho-Galvanic Studies on Sixty-four Medical Students. By C. HANS SYZ 54

Publications Recently Received 70

Proceedings of the British Psychological Society . . . 79

Eighth International Congress of Psychology . . . 80

British Psychological Society, Names of Officers and Members *at end*

British Psychological Society Rules . . . . . . *at end*

PART 2. OCTOBER, 1926

✓The Correlation between Intelligence and Size of Family. By H. E. G. SUTHERLAND and GODFREY H. THOMSON . . 81

The Constancy of 'g,' General Intelligence. By C. S. SLOCOMBE 93

Cadence and Phrase Tests in Music. By H. LOWERY . . 111

Observations on the Unreliability of Subjective Reports of Emotional Reactions. By C. HANS SYZ 119

A Method for Measuring Simultaneous Contrast. By J. A. VAN HEUVEN 127

A Suggestion towards a New Hypothesis regarding the Localization of Sound. By H. BANISTER 142

Critical Notice of Head's *Aphasia and Kindred Disorders of Speech.* By F. C. BARTLETT 154

Publications Recently Received 162

Notes on Recent Periodicals 165

PART 3. JANUARY, 1927

PAGE
Pleasure, Pain and Conation. By WILLIAM McDOUGALL . 171
The Development of Sentence Structure in Childhood. By WILLIAM BOYD 181
An Experimental Study of a Repetitive Process. By S. WYATT 192
Experiments on the "Direction of Associations" in Young Chickens. By G. C. GRINDLEY 210
A Brief Theory of the Organism Suggested by an Experiment on the Perception of Almost Inaudible Sounds. By R. W. PICKFORD 222
The Tetrad-Difference Criterion. By GODFREY H. THOMSON . 235
Prof. Bernard Muscio. Obituary 256
Preliminary Report on a Demonstration of Experiments on Hypnotism shown by Gustaf Wallenius, Fil. Mag. (Upsala) 257
Publications Recently Received 258
Notes on Recent Periodicals 263
Proceedings of the British Psychological Society . . . 266

PART 4. APRIL, 1927

An Experiment for the Testing of the Hypothesis of Lamarck. By WILLIAM McDOUGALL 267
The Creative Process in the Artist's Mind. By S. ALEXANDER 305
Material *versus* Abstract Factors in Correlation. By C. SPEARMAN 322
A Group Factor in Sensory-Motor Tests. By ERIC FARMER . 327
Parallelism in Curves of Motor Performance. By ERIC FARMER 335
The Relation between Intelligence and Social Status among Orphan Children. By D. CARADOG JONES and A. M. CARR-SAUNDERS 343
Critical Notice of *The Measurement of Intelligence*, by Professor E. L. Thorndike and Staff. By C. SPEARMAN 365
Critical Notice of *Psychology and Ethnology*, by Dr W. H. R. Rivers. By BRENDA Z. SELIGMAN 370
Publications Recently Received 377
Committee for Research in Education 380
Proceedings of the British Psychological Society . . . 383

LIST OF AUTHORS

PAGE

Alexander, S. The Creative Process in the Artist's Mind . 305

Banister, H. A Suggestion towards a New Hypothesis regarding the Localization of Sound 142

Bartlett, F. C. Critical Notice of Head's *Aphasia and Kindred Disorders of Speech* 154

Boyd, William. The Development of Sentence Structure in Childhood 181

British Psychological Society, Names of Officers and Members *at end*

British Psychological Society Rules . . . . . . *at end*

Committee for Research in Education 380

Davey, Constance M. A Comparison of Group Verbal and Pictorial Tests of Intelligence 27

Drummond, Margaret. The Nature of Images 10

Eighth International Congress of Psychology . . . 80

Farmer, Eric. A Group Factor in Sensory-Motor Tests . 327

Farmer, Eric. Parallelism in Curves of Motor Performance . 335

Gray, P. L. and Marsden, R. E. The Constancy of the Intelligence Quotient—Final Results 20

Grindley, G. C. Experiments on the "Direction of Associations" in Young Chickens 210

Heuven, J. A. van. A Method for Measuring Simultaneous Contrast 127

James, H. E. O. Regularity and Rhythmicalness . . 1

James, H. E. O. and Thouless, R. H. A Note on the Effect of Polarisation in Psycho-Galvanic Experiments . . 49

Jones, D. Caradog and Carr-Saunders, A. M. The Relation between Intelligence and Social Status among Orphan Children 343

Lowery, H. Cadence and Phrase Tests in Music 111

McDougall, William. Pleasure, Pain and Conation . . 171

McDougall, William. An Experiment for the Testing of the Hypothesis of Lamarck 267

PAGE

Notes on Recent Periodicals 165, 263

Obituary: Prof. Bernard Muscio 256

Pickford, R. W. A Brief Theory of the Organism Suggested by an Experiment on the Perception of Almost Inaudible Sounds 222

Preliminary Report on a Demonstration of Experiments on Hypnotism shown by Gustaf Wallenius, Fil. Mag. (Upsala) 257

Proceedings of the British Psychological Society . 79, 266, 383

Publications Recently Received 70, 162, 258, 377

Seligman, Brenda Z. Critical Notice of *Psychology and Ethnology*, by Dr W. H. R. Rivers 370

Slocombe, C. S. The Constancy of 'g,' General Intelligence . 93

Spearman, C. Material *versus* Abstract Factors in Correlation 322

Spearman, C. Critical Notice of *The Measurement of Intelligence*, by Professor E. L. Thorndike and Staff. . . . 365

Sutherland, H. E. G. and Thomson, Godfrey H. The Correlation between Intelligence and Size of Family . 81

Syz, Hans C. Psycho-Galvanic Studies on Sixty-four Medical Students 54

Syz, Hans C. Observations on the Unreliability of Subjective Reports of Emotional Reactions 119

Thomson, Godfrey H. The Tetrad-Difference Criterion . 235

Wyatt, S. An Experimental Study of a Repetitive Process 192

VOLUME XVII JULY, 1926 No. 1

THE BRITISH
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GENERAL SECTION

REGULARITY AND RHYTHMICALNESS[1]

BY H. E. O. JAMES.

(*From the Psychological Laboratory, Manchester University.*)

1. *Introduction and definitions* (pp. 1, 2).
2. *Initiating a rhythm* (pp. 2, 3).
3. *Interference of rhythms* (pp. 3, 4).
4. *Primary and secondary aspects of rhythm* (pp. 4–6).
5. *Perseveration of rhythmical patterns* (pp. 6, 7).
6. *Regularity and rhythmicalness in a kinaesthetic series* (pp. 7, 8).
7. *Appendix* (pp. 8, 9).

1. *Introduction and Definitions.*

THIS paper contains a brief account of an experimental attempt to elucidate the relations between regularity and rhythmicalness. For the sake of conciseness and clarity the experiments themselves are described summarily and only in part, chiefly in order to illustrate a certain theory of rhythm. But in spite of the somewhat deductive mode of exposition, it should not be forgotten that the theory is subordinate to and the outcome of experiment. Whether the view of rhythm expressed is generally valid or not, is not discussed here. The one test employed is that of its applicability to the facts of the relationship between regularity and rhythmicalness. It is true that there is other evidence in favour of this theory, but even a summary of this must be postponed.

It will conduce to clearness if fairly precise notions of what regularity and rhythmicalness are to signify in this paper are obtained at the outset. Regularity may refer either to experience or to the conditions of experience. It may denote either the degree of uniformity with which a given experience is repeated or the degree of uniformity with which the conditions of a given experience are repeated. Further, just as an experience or the conditions of that experience have many aspects, so too has regularity in either sense.

---

[1] This paper was read at the Southampton Meeting of the British Association in September, 1925.

That an experience is rhythmical signifies here that the experience in question is of such a kind as to be judged rhythmical by the experiencer. Rhythmicalness thus refers solely to experience. Such expressions as rhythm-inducing or rhythmogenic conditions can be used to indicate the conditions of rhythmical experience.

Very little reflection and experiment are necessary to show that regularity, even complete regularity, does not always involve rhythm. A regularly repeated experience is not invariably rhythmical, nor does a regularly repeated set of stimuli necessarily induce an experience of rhythm. Regularity is thus not the sole condition of rhythm. One must therefore ask what is the effect of variations in regularity upon the rhythmicalness of a series of experiences.

An experience of rhythm can be described as an experience of a series of successive groups of approximately uniform pattern, each group containing one or more accents and separated from its neighbour groups by pauses that are not too long and not too irregular. The pattern of a group may be described provisionally as the mode of arrangement and constitution of a number of grouped salient or prominent phases of experience, *e.g.* sounds, separated by phases of less salient character, *e.g.* silences. The salient and less salient phases have definite positions in the pattern and are more or less differentiated in respect of quality, intensity, duration, salience, etc. An accent, for example, is differentiated from the other phases of the group at least in regard to salience, it is in a group containing one accent the most salient phase in the group. In groups containing more than one accent the accents themselves often differ in salience.

The determination of what constitutes the pattern of a rhythmical series is of course a matter for experiment although it has been necessary to anticipate. A promising method is that of ascertaining whether or not the rhythmicalness of a series is impaired by irregular variation of particular aspects of the pattern. The question of regularity thus narrows itself down largely, although not altogether, to a question of uniformity of pattern.

2. *Initiating a Rhythm.*

It is convenient to suppose that the process of initiating a rhythmical series is that of the arousal to activity of a group-disposition of a specific pattern, and that the business of maintaining the rhythmicalness of the experienced series is, when once this disposition is functioning efficiently, pre-eminently that of maintaining the efficient and undisturbed functioning of this disposition and of no other, or in other words of ensuring the continuous conditioning of experience by this disposition and by no other. Phase by phase the active disposition conditions the successive phases of the experienced group-pattern, then after a pause the same process is repeated. To use an analogy, the activity of the disposition is like that of the mechanism controlling the orderly production of the successive component lights of a sky-sign, whilst the experienced pattern is analogous to the lights as they come and go in their orderly sequence.

The period of initiation of a rhythmical series varies according to the nature of the pattern and according to the other conditions of initiation. The more complex or novel the pattern, the longer before the relevant disposition is functioning efficiently and the longer before the maximum of rhythmicalness consistent with the given pattern is attained. This phenomenon of warming-up to a rhythmical pattern is familiar to most.

3. *Interference of Rhythms.*

But there may be another reason for delay in getting into the swing of a rhythm. This is interference by another disposition. If, for instance, the pattern of a previous rhythm perseverates whilst the new pattern is being initiated, then the period of initiation lengthens. We may suppose that there is conflict between the nascent and perseverating dispositions. Sometimes rivalry manifests itself—there is an alternation in experience between the two patterns as now the one disposition prevails and now the other. This conflict between dispositions helps us to understand one way in which irregularities can impair rhythmicalness.

Generally speaking, two things may happen when the conditions are so modified as to arouse a disposition other than that which up to this point has been functioning alone. Firstly, the new conditions may wholly favour the new disposition. In this case a rhythmical series of the new pattern is successfully initiated. If, for instance, the following series of auditory stimuli and intervals is repeated: a sound of 0·20″, an interval of 0·80″, a sound of 0·06″, and an interval of 1·14″, the rhythmical pattern experienced is Long Sound-Short Sound. But if the positions of the two intervals in the sequence are interchanged, the outcome is a series of reversed pattern: Short-Long, after a brief transitional period.

Secondly, there are those cases in which the new conditions irregularly favour different patterns. Here, provided that the irregularity is frequent enough, annihilation of rhythmicalness occurs. The experience in this case is not merely one which lacks rhythmicalness. It is disagreeable and characterised by frustration, just such an experience as might be expected to result from conflict of an acute kind. An example of such destruction of rhythmicalness could be obtained by irregular interchange of the positions of the intervals in the above series. (It may be as well to add that if this interchange were regular, the result would be a rhythmical series of a pattern consisting of four or six or more sounds. This case really falls into the first class.)

Similarly it can be shown that irregular variation of the sequence of the durations of the salient phases or of their number in the pattern is destructive of rhythmicalness. It is probable, although this has not yet been experimentally established, that a similar statement holds of the intensity, quality and accent sequences.

4. *Primary and Secondary Aspects of Rhythm.*

It seems proper to call such aspects of the pattern primary aspects. For if they are varied, either a new pattern results or rhythmicalness disappears. And just as a change of pattern can be attributed to the arousal of a new disposition by the changed conditions, so the disappearance of rhythmicalness can be attributed to the simultaneous activity and consequent conflict of two or more dispositions, none of which is able to prevail. It is not sufficient to say that the conditions for rhythm have disappeared. Merely to say that would be to leave unexplained the disappearance of rhythmical experience despite the well-known strong perseverative tendencies of rhythmical patterns. For the usually applicable explanation of the cessation of rhythmical experience, namely, that the subject has reacted to another situation, will not answer here owing to the subject's pre-occupation with matters rhythmical. Further the subject's experience is not related to rhythmicalness by mere negation as is the case when reactions to non-rhythmogenic situations are made. The relation is rather that of noise to musical tones than that of sound to non-auditory experience.

It is clear that changes of the primary aspects of the pattern are all or none changes. Either the order or the number of the phases is changed or it is not. But it is easy to make changes in the pattern which are not only capable of gradation but also condition a gradual variation of rhythmicalness. The duration of an interval or of a sound can, for instance, be varied within quite wide limits whilst yet the interval or the sound in question remains less or greater than those sounds or intervals which it was initially less or greater than. The result of irregular variation of this kind is a diminution of the rhythmicalness of the series, a diminution gradually increasing with the magnitude of the irregularity. And this gradation persists without break so long as changes of this kind do not bring about changes of the primary aspects. These aspects of the pattern which admit of gradual variation and which can condition variations of rhythmicalness, may be termed secondary aspects.

To illustrate this we may begin with a rhythmical series conditioned by the following sequence of intervals and sounds: Long-Short = 0·80″, Short-Long = 1·14″. The pattern of the series would be Long-Short. If now we begin irregularly to alternate the above sequence with the following sequence: Long-Short = 0·60″, Short-Long = 1·34″, the same pattern as before, Long-Short persists, although with diminished rhythmicalness. If, on the other hand, we begin with the following sequence: Long-Short = 0·88″, Short-Long = 1·06″, the pattern experienced will be Long-Short. But if this sequence is irregularly alternated with the following sequence: Long-Short = 1·06″, Short-Long = 0·88″, the series conditioned thereby is quite unrhythmical. For in the latter case a change of a primary aspect is involved whilst in the former case only a secondary aspect is modified. Clearly it is not the magnitude of the change that is in question, for the variation in the two cases is approximately the same, and it would be easy to make the variation in the former case distinctly the larger without producing a complete fading-out of rhythmicalness.

Contrasting the effect of irregularities in secondary aspects with the effect of irregularities in primary aspects warns us that the impairment of rhythmicalness by irregular variation of secondary pattern-aspects must not be ascribed to the conflict of simultaneously active dispositions. The first step towards explanation is taken when it is realised that an active disposition must have a certain capacity for adaptation, it must be able to adjust itself to irregularities, it must as it were possess a certain degree of tolerance or elasticity. But the effect of this necessity for re-adjustment, of this recurring demand upon the disposition's tolerance, must be to set up a strain. And this strain, it may be supposed, affects experience by way of a diminution of rhythmicalness. The following will perhaps make this hypothesis more plausible.

If the sequence Long-Short = 0·80″, Short-Long = 1·14″ alternates regularly with the sequence Long-Short = 0·60″, Short-Long = 1·34″, each sequence being presented once in turn, the result is not a series of diminished rhythmicalness with the pattern Long-Short, as might perhaps have been expected, but a quite rhythmical series in which the pattern consists of four sounds instead of two. Yet when the alternation was irregular, the pattern of two sounds was quite capable of maintaining itself although at the price of diminished rhythmicalness. It seems legitimate to suppose that the appearance of the larger pattern enables the strain of frequent re-adjustment to be avoided. Otherwise it is difficult to account for the change of pattern in view of the tendency to perseverate of rhythmical patterns.

The capacity for re-adjustment or the adaptability of a disposition can be shown in another way. If, for instance, the subject is warmed up to the following sequence: Long-Short = 0·40″, Short-Long = 1·54″, and if then, at every other repetition of the sequence, the longer interval is decreased and the shorter increased by 0·02″, it is possible to continue this progressive equalisation until the two intervals are equal, and this without any diminution of rhythmicalness being reported by the subject. The subject becomes aware after the change has proceeded far enough that the magnitudes of the intervals have changed, but still rhythmicalness is unimpaired. It may be supposed that the disposition has progressively modified itself, otherwise the cumulative change would have resulted in a strain and a diminution of rhythmicalness.

5. *Perseveration of Rhythmical Patterns.*

Some other points in regard to rhythmical dispositions are perhaps worthy of mention. It is clear that it would be easy to explain the phenomena of the perseveration of rhythmical patterns as the effect of the inertia of active dispositions. With the more common instances of rhythmical perseveration I shall not deal here. But there are two cases worthy of mention.

Suppose we have a series of sounds at equal intervals, every other sound being the longer. If the series is begun with the longer sound, the pattern is Long-Short. But if the series is begun with the shorter sound, the pattern experienced is Short-Long. It is clear that the disposition first aroused persists in activity and inhibits the disposition corresponding to the alternative pattern, although in all respects save in that of priority of arousal the conditions are equally favourable to both dispositions. This is manifestly an inertia effect.

The second case is partly an effect of inertia, partly of modifiability or capacity for re-adjustment. If we begin with a series in which the Long-Short is less than the Short-Long interval, the pattern will be Long-Short. If after warming up to this pattern for a minute or so, the Long-Short interval is progressively increased and the other interval decreased by sufficiently small steps, it is possible to make the Long-Short interval longer than the other interval without a reversal of pattern taking place. Yet if we begin instead of ending with this state of affairs, the pattern is Short-Long. Of course if the difference between the intervals is made sufficiently great, the pattern changes and the pattern favoured by the relative magnitudes of the intervals prevails. Nevertheless it is clear that the inertia and the modifiability of the disposition first aroused have made it possible for one pattern to persist when the remaining conditions favour another pattern.

It remains to be added that the secondary and primary aspects of the pattern so far described have been almost exclusively intra-pattern aspects. But if the intervals or pauses between the successive group-patterns are irregularly varied as regards duration, the result is impairment of rhythmicalness. Such aspect of the pattern can be termed inter-pattern aspects or perhaps better inter-group as distinguished from intra-group, for strictly speaking it is possible to consider the intervals between the groups as partly constituting the pattern.

Auditory rhythmical series have been used as examples in the foregoing, not only because many of my experiments were carried out with them but also because it is possible to control the amount and kind of irregularity in series of auditory stimuli. Similar possibilities of control seem to be promised by visual series, but with these I have not worked to any great extent. Motor or kinaesthetic series—the other kind of series with which I worked—are not controllable to any useful degree in regard to regularity. Hence in order to investigate the relations between regularity and rhythmicalness in such series more laborious and less direct methods must be adopted. The method adopted was that of comparing series by pairs in respect of rhythmicalness.

6. *Regularity and Rhythmicalness in a Kinaesthetic Series.*

If, other things being equal, rhythmicalness varies as regularity in rhythmical series, then of two compared series that which is reported to be the less rhythmical should be the less regular. Further the differences in respect of regularity should be less on the whole in the case of series reported to be equally rhythmical than in the case of series reported to be unequally rhythmical. If then kinaesthetic rhythmical series can be produced and compared by pairs as regards rhythmicalness, if the temporal regularity of such series can be measured with a sufficient degree of accuracy, and if the results agree in the main with the stated expectations, then it may be taken that on the whole an increase or decrease in the temporal regularity of a kinaesthetic rhythmical series conditions an increase or decrease of the rhythmicalness of the series. It should be noted, however, that the converse proposition is not only unproven but also untrue—save under special conditions. It would not be true to state generally that an increase of rhythmicalness is due to an increase in temporal regularity, not only because temporal regularity is not the only kind of regularity but also because regularity is only one of the conditions of rhythmicalness and not the sole condition. In these circumstances it is clear that even if the experimental conditions are kept as constant as possible, it cannot be expected that the stated expectations will be more than roughly verified.

The subjects were instructed to render a specified rhythmical pattern by moving the forefinger of the right hand up and down. This finger was attached to one end of a pivoted rod, the other end of which being provided with a pointer recorded the movements of the finger on the smoked surface of a rotating drum. After two series had been performed and recorded in this way, the subject was asked to state whether the two series were equally or unequally rhythmical, and if the latter, which of the two was the more rhythmical. Cases of uncertainty were to be treated as cases of equality. The records of the movements were afterwards measured with a view to determining the degree of irregularity of each of the series. The measurements were made to the nearest fiftieth of a second. Four subjects were used, the number being small on account of the large amount of time required for preliminary practice.

The results obtained—a brief summary is given below—showed clearly enough on the whole that firstly the less rhythmical series were less regular than their more rhythmical partners, and secondly that the differences in regularity between the members of a pair were greater in cases of unequal than of equal rhythmicalness. It seems permissible to conclude that an increase or decrease of temporal regularity conditions an increase or decrease of rhythmicalness in kinaesthetic rhythmical series.

7. *Appendix.*

In the condensed frequency table given below the percentage differences in irregularity between the members of pairs of compared series are stated. In the case of inequality judgments this difference is positive when the more rhythmical is the more regular, negative when the more rhythmical is the less regular. It is clear that there are more ways than one of determining the temporal irregularity of a recorded series. Three methods were used, all of which gave substantially similar results. The results of only one of these methods is given below.

It would take too long to give details of the methods of measuring and calculating the temporal irregularity of a series. Essentially they consisted in dividing the pattern into parts, the number of parts varying with the pattern, in obtaining the mean and the mean variation for each of these parts in the series, in expressing the mean variations as percentages of the corresponding means and in taking the average of these percentages. This average was taken as a measure of the temporal irregularity of the series.

Equality pairs. Frequency	Differences of irregularity	Inequality pairs. Frequency
	6·0 to 6·9	2
	5·0 to 5·9	6
	4·0 to 4·9	5
1	3·0 to 3·9	22
3	2·0 to 2·9	16
20	1·0 to 1·9	17
38	0·0 to 0·9	3
	−1·0 to −0·1	3
	−2·0 to −1·1	2
	−5·0 to −4·1	1
	−6·0 to −5·1	1
$N=62$.		$N=78$.

THE NATURE OF IMAGES

BY MARGARET DRUMMOND.

1. *Percept and image* (pp. 10, 11).
2. *Experimental procedure and results* (pp. 11–15).
3. *Eidetic images* (p. 15).
4. *Development of imagery in an individual* (pp. 16, 17).
5. *Mode of production of image* (p. 17).
6. *Eidetic imagery and children's drawings* (pp. 17, 18).
Function of the eidetic image (pp. 18, 19).
Conclusions (p. 19).

1. *Percept and Image.*

IN practice the distinction between percept and image is usually made with ease, but it is not so easy to agree upon the terms in which the difference may be expressed. Ribot's theory, which was founded on that of Bain, is thus summarised by himself: "The ideal of every recollection is that, while keeping its character of being already experienced, it should be adequate in such measure as was possible for the original expression. The revival of impressions is an internal operation, whose extreme form is hallucination." The position taken up apparently is that images are 'faint' percepts, and that as they increase in force or vividness they more and more nearly approximate to the perceptual process. This view is probably based upon Ribot's acceptance of Bain's doctrine of the cerebral activity accompanying the imaginative process. "The renewed feeling" (*i.e.* the image), says Bain, "occupies the very same parts and in the very same manner as the original feeling, and no other parts and in no other assignable manner."

No proof is forthcoming of this physiological dogma; we may therefore set it aside without discussion. That the image is a 'faint' percept is, however, a doctrine that still holds its own in psychological text-books in spite of the mass of reliable introspective testimony to the vividness of mental imagery.

Individual differences play so great a part in psychology and neglect of this fact has led to so many disputes that I shall begin by describing what seem to me to be in my own case the most important differences between image and percept. Except when the contrary is expressly stated I shall be referring to visual imagery, as it is most common, and in the case of some people appears to be the main vehicle of memory and of the various forms of thinking.

1. When I attend to an image my attention is directed towards an inner world. It makes little difference to me whether my eyes are open or shut: in neither case do I see what is in front of them. When children speak of their mental pictures as being 'in their head,' I think they may refer to this withdrawal of attention from the external environment.

2. My usual thought processes are not visual, but verbo-motor. Visual images occasionally occur spontaneously as an accompaniment or assistance to the course of thought. They are fairly vivid in form and colour, but I have reason to think not nearly so vivid as those of many other people. They may be stationary or they may move. They are just as much or as little subject to my will as thoughts which are not expressed in image form. When I create an image for purposes of observation it may continue to haunt me for some time. Holding an image in the focus of my attention does not as a rule give me increased vividness or increased detail.

3. Although my images may truly be described by the term visual they are never in ordinary circumstances before my eyes. They are definitely *not* on the field of darkness when my eyes are closed, nor are they projected into perceived space, as an after-sensation, for example, is, when my eyes are open.

4. My dream life is conducted more in terms of visualisation than my inner waking life, but on the whole I am not sure that the images differ in character. I have occasionally had a vivid auditory or touch hallucination in a dream, but I do not think the visual images are hallucinatory in character.

5. My images do appear to stand over against the cognising mind as objects; upon them the act of apprehension seems to me to be directed. Yet I am quite certain that there is not a nucleus of something actually present to the senses upon which the act of discriminating is originally directed. I am quite certain that my experience does not consist of a penumbra of imagery gathered round such an object[1].

2. *Experimental Procedure and Results.*

In the endeavour to find out whether images are commonly by some people localised in perceived space, and whether in such cases the image is built up round a sense nucleus, I gave thirty-four students of psychology each a sheet of paper, and asked them if they could create an image and throw it on the paper. Thirty-three said they could; only one reported failure. I asked them to say anything they could about the image. In response to this it appeared that one or two of them were helped by elements of light and shade in the paper—which gives countenance to the theory that there is a sense nucleus, but in most cases this was not admitted. The great majority of students were certainly not aware of receiving assistance in their construction from sense data. Twenty said that the image was flat like a picture; seven that it was solid in appearance. I then drew a cross on the board and asked the class to project an image on to the board and state whether it concealed the cross. Eight declared that it did do so. On another occasion forty-three students (not the same) were asked to draw anything they liked and then to report on the mental processes which accompanied the task. Two of them spontaneously reported images which came into perceived space. One of these says, "While I was thinking what I would draw, I got a very clear vision of a black oblong box. I was looking on the desk in front of me and I saw the image quite distinctly *on the desk*. I was about to draw a crocus corm when the vision appeared." The other drew a cup and saucer. She says with reference to her visualisation of the cup, "One would actually have thought that a white cup was coming *out of the paper*." In both those cases it is clear that there might have been a sense nucleus though it is not so stated.

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[1] Cf. G. Dawes Hicks, "On the Nature of Images," this *Journal*, xv, 133–4.

After these trial experiments I picked out one or two people who seemed to have the power I was looking for in a marked degree and whose introspection seemed reliable. I interviewed each subject alone.

Method of experiment. I placed a cartoon from *Punch* in front of subject *A*, a student taking a second year's course in psychology at a Training College. I allowed her to look at it for thirty-five seconds; I then withdrew it and asked her to look at a blue-grey sheet of paper and see whether the picture would appear on it. The cartoon, representing an Indian filling the pipe of peace, was one by Bernard Partridge. I had selected it partly because on the flap of the tent behind the seated figure are the words "Washington Conference," and on the tobacco tin at his feet are the words "ALL ROUND NAVY CUT PREPARED BY THE WARREN G. HARDING CO. U.S.A."

Subject's introspection and behaviour. The subject's behaviour confirmed her statement that she really saw the picture on the background provided. There were long pauses during which she appeared to study the paper intently. The picture, she said, fluctuated, being sometimes clearer, sometimes less clear. At first came the picture of the Indian slightly...."The whole picture's coming." Long silence. (Can you make out letters?) "On the tin ALL OUT and Partridge—more written in this corner....Hills at back....Picture of some fowl on the tent." (Look longer.) "There's a pipe. The man looks as if he were filling it. A round tin like a frying pan at his feet. Flap of tent is open....Virginia, I think, is printed on the tin."

This last statement is incorrect; the other items are correct.

I now repeated the experiment with a coloured picture. The mat was studied attentively for about thirty seconds before any description was attempted.

"Girl sitting at piano, her hands on the keys...fair-haired boy beside the piano...girl standing at the front probably singing with a yellow fan in her hand and a blue dress with white spots...group of girls standing behind, some with yellow and some with red dresses on and one sitting with a yellow fan in her hand....Probably a picture hanging on the wall, but it isn't a picture—it's pink....A chandelier in the room...a bowl of flowers on the piano right in the very front...brilliant colours...girl at piano has a pink dress on—she's facing the front—not looking at the piano." In reply to a question the subject said that this had not been clear at first: "she's become much more clear."

The subject's attitude again was that of one studying a picture actually before her eyes. The statements made are all correct, and it will be seen that colour terms are freely used. The whole interview took about half an hour.

I tried this same experiment with a colleague, a graduate in science (*B*), a marked visile. Whereas *A* seemed passively receptive to what came on the projection mat, *B* declared that she put the form on the paper with a considerable amount of effort. She was troubled by the grey tone of the paper; she obtained a better representation when creamy paper was substituted; she preferred the paper vertical to lying flat on the table; she found that half closing her eyes and shading them with her hands so as to shut out stimuli from surrounding objects improved the image. Concentration on any part made the details of that part come out more clearly. I asked about the letters on the tin. "I get only the big ones. I can see NAVY CUT as if I was reading it—not the smaller letters. I see CO." *B* found that the image projected on the paper had nothing like the clearness it has "when I'm not looking at anything." In the latter case "it is very real—in fact it rather becomes alive. I can't think of it as a picture lying upon the bench. I think I'm really thinking of it as a solid sort of thing." I asked if she could introduce motion. "Oh, yes, quite well. I can even make him rise. I can see the fringe to his jacket moving. You know how it would swing." I asked if she could introduce colour, and received a similar affirmative reply.

In attempting to form an image of the coloured picture *B* found masses of colour appearing on the mat; when she concentrated on one item the others disappeared. Half closing her eyes improved the appearance.

Subject *C*, a student in her second year, obtained the picture on the mat very readily. I tried bending the background. She reported that her picture bent with it. She could see a very slight movement of the Indian's eyes as if he were watching, also a movement of his hands, but this she felt within herself rather than saw; she described the hand movement with the aid of gestures. Of a coloured picture of a girl descending some steps, shown for twenty seconds, *C* obtained a very full reproduction on the mat; she described her image, pointing to the different parts of the paper on which she saw the various items. At one point she said, "Another picture seems to be coming in; it seems to be fighting with this one." I drew an asterisk on the blackboard and enquired of *C* if she could make her image of the girl hide this drawing just as my hand hid it when I placed it in front. She said, "Yes, but not for long; she goes away and wont come back."

Subject *D*, a colleague, obtained the picture in great detail. With her it came up as a whole; concentration did not seem to cause it to develop. She was able to fit her image into the panel of a door as if within a frame. She did not seem to make use of the sense data as a nucleus for her picture. She was able to place the image of a white cup in front of the asterisk on the board, but she could not quite obscure the asterisk.

Subject *E*, another colleague, who I thought might have this power of projection of visual images, because she told me she had such a good memory for anything she had seen, even if she had not paid attention to it at the time, failed to place an image on the mat. Her memory of the details of an experience came with marked spatial references, but she was not sure how far she was visualising.

Many people who do not as a rule project their images may experience the phenomenon in the dark just before going to sleep. A student reports as follows: "I have found that although I cannot direct my images into space during the day, I can do so in the firelight or when it is almost dark. If I just sit and think for some time, the image seems to come up before me out of the darkness, and the more I dwell on it, the plainer it becomes. I had a very vivid experience of that a few nights ago. I had been almost asleep, then wakened again, and was lying looking at the ceiling. Between myself and the ceiling, an image seemed to take form, so real that I could almost have believed it to be alive. I wondered if it would move if I turned my eyes. I did so, and it moved across the ceiling to the window, and out of my sight. It was the most vivid experience I had had of such a thing."

Though I do not think that Dr Dawes Hicks is right in thinking that images are usually formed round a nucleus of sense data, yet I think this subject may be making use of such data in the instance she gives. An account she writes of a sound image seems to suggest an analogous mode of procedure. "As a rule," she says, "I know exactly what a sound would be like, and all about it, but somehow I cannot exactly hear it. Just now I thought of the breaking of great sheets of ice in a river. At first I could not hear the noise, then suddenly for a moment there was a noise outside the room, which seemed to fit in exactly with my image, and then I heard the breaking and smashing of the ice. The image was quite clear as long as I chose to think of it." Here actual perception served to initiate imagery, as it often does in dreams.

3. *Eidetic Images.*

These images localised in perceived space seem to me to be the same as those recently described by the Marburg school of psychologists under the name Eidetic Images. Their experiments have been tried for the most part on children. Mine, which have extended over a good many years, though of the earlier ones I did not keep systematic records, have been tried for the most part on students in the late teens or early twenties. After reading Mr Allport's paper on "Eidetic Images[1]" I tried seven London children in the way described above. Of these, two quite definitely found that looking at the projection mat hindered the formation of an image. The other five possibly did obtain a picture on the paper, but it was certainly very poor in colour, form, and detail compared with the original. Mr Allport reports cases where the eidetic image gave more than one would think could be observed during the thirty-five seconds allowed for study of the picture; for example, he says, "I found that a number of children, whom I examined, were able to spell out correctly, or almost correctly, from their image the word *Gartenwirthschaft*, which was for them quite meaningless."

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[1] This *Journal*, Oct. 1924.

4. *Development of Imagery in an Individual.*

The Marburg investigators regard the eidetic image as a primitive form. Indeed one theory is that both perception and the memory image arise out of the eidetic phenomenon. Mr Allport accepts the theory that the subject belongs to the sphere of child psychology. There is said to be an observable retreat of the ability during adolescence. Further study of the subject is required before we can fully accept these contentions and theories.

One may seek for further evidence in the matter by studying the development of imagery in the child. Some twelve years ago the entrance into the family of a baby who rapidly developed into a very bright imaginative child provided me with the opportunity I desired. From about her fourth year M. spoke of imaginary companions and pets, but I was not able to persuade myself that her occasional claim to see them was to be interpreted literally. At this stage her introspective testimony seemed to me thoroughly unreliable, and her categorical assertions seemed to me to be the expression of wishes rather than a description of facts as they appeared to her. At six years of age she seemed able to adopt a more scientific attitude to my questions. She now denied having any visual images except "coloured balls" which she saw when she shut her eyes, and which seemed to me to be of the nature of those images on the field of darkness described by Ladd and others[1]. At twelve years of age M. claims to have mental pictures, but according to her own account she knows what should be there first and then puts that into her mental image. She was one of the seven children referred to above. When she was studying the picture shown to her, I noticed that she was describing it to herself in words. She told me afterwards that she thought I was going to question her about it, and did this to help herself to remember. Her description was much more full than that of any of the other children, but she entirely failed to obtain a picture on the projection mat. She thought there were eight or nine men in the picture. I said, "Why *think*?" She said, "I'll count again—eight." There were nine, but ability to count seemed to indicate a clear image. She saw the sky blue, and "clouds seemed to come of themselves." She could introduce movement into the image.

Dr Dawes Hicks appears to accept the well-known fact that children create imaginary playfellows for themselves as evidence that they realise their images in the external world. In spite of many years' endeavour

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[1] See *Elements of Psychology*, Mellone and Drummond, pp. 405–10.

I have not yet found evidence that this is the necessary interpretation of their conduct. In some cases the images may be so realised; in others I am convinced that they are not.

5. *Mode of Production of Image.*

In the evidence with regard to the way in which the images arise, there are facts that suggest that there may be two modes of origin. First, the subject may remember a detail and insert it into his image. Second, the subject may adopt a passive attitude towards his picture which is automatically enlarged and enriched as he continues to look. Observer *B* belongs to the first type, so far at least as the eidetic image is concerned, observer *A* to the second. M. also belongs to the first type, but the clouds' coming "of themselves" into her picture indicates that the factor of automatic development may also be operative. Freud in his psycho-analytic procedure counts largely on this automatic reinstatement of the past. People who can see in magic mirrors, crystals, etc. are probably *eidetiker* who have developed this automatism. Actual perception is a powerful agency in causing this automatic reinstatement of the past. Evidence of this is readily available from people who after many years have revisited a place once familiar to them.

Many people whose imagery is ready and varied give themselves over to the enjoyment of images that come automatically in day-dreams or in reading poetry or stories. These images frequently are representative of incidents in the past of the subject and are often not at all appropriate; in the case of reading they are often misinterpretations of the matter read. One can scarcely doubt that many children are the victims of this automatic functioning of the associative process. The false tales told in good faith by many children are evidence of this.

6. *Eidetic Imagery and Children's Drawings.*

Further evidence for the existence of eidetic imagery—a matter which to some minds is still not established by the testimony adduced—may be sought in the study of children's drawings. At one time I fancied that the children who draw superlatively well at a very early age and with hardly any instruction might be *eidetiker*. Any evidence I have, however, seems rather against that hypothesis. It is, I think, possible that with some of those children drawing is a mode of expression which to some extent takes the place of visual images. Many observers have denied the presence of visual imagery when drawing a simple design from memory, or when drawing something they were in the habit of drawing. If the visual image is tri-dimensional, it would be quite as difficult to draw from as a percept—a thing which many people find extremely hard. Moreover, if an image is projected on the paper, it is apt to disappear when attention is concentrated on the drawing activity. Two years ago I received from the teacher of an infant class a document which is of interest in this connection. The original was made by a girl (age, five years, nine months) on a blackboard with white chalk and was copied by the teacher. I give a tracing of what she sent me (Fig. 1). The child could not read what she had written, her powers being limited to knowledge of the letters and ability to read *very* simple words. The legend is a "co-operative advertisement" and most of the children of the place spend hours in what is known as "the Store."

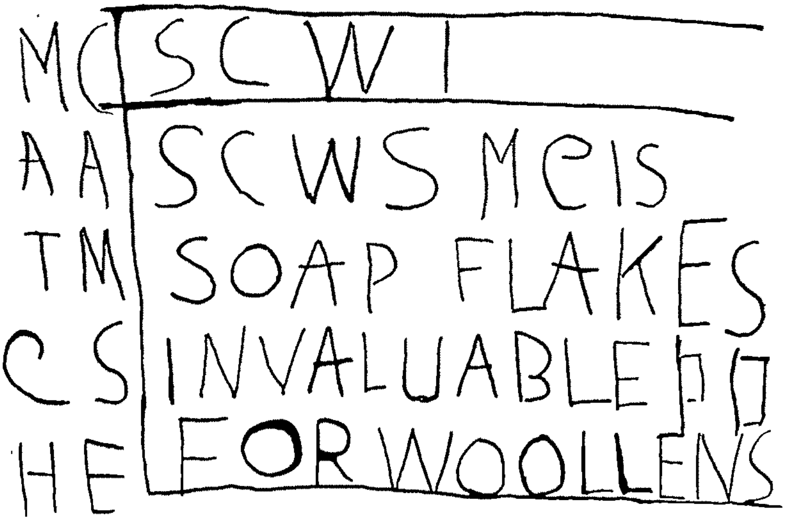

Fig. 1.

This drawing may represent a visual memory, and if so there may have been an eidetic image. On the other hand when the child was waiting for her mother, she may have murmured those letters over and over to herself and so have memorised them. Her order is quite correct, and some psychologists maintain that auditory or motor images are more apt to give correct order than visual ones.

Function of the Eidetic Image.

Mr Allport believes that the function of the eidetic image is to preserve the stimulus situation for the child so that he may practise and perfect his responses. It is claimed that the image may give more than the perception did. This 'more' may be a true representation of the actual situation or may be false—a fact which seems to detract from its value. It is an established fact that memory is often better after an interval than immediately. But I have not personally been able to obtain any evidence that the eidetic image is richer than the perceptual experience nor that it differs except in localisation from the ordinary memory image.

There are, however, on record certain facts which may be cited in favour of the theory that the memory image may contain what has not been noted in the act of perception. Jastrow gives a case of a girl seeing in a crystal a friend in a certain situation which was true to fact, but which the seer did not recognise as a memory. It was afterwards shown that the actual scene portrayed in the crystal had been before her eyes, though she had not marked it. Is it possible that the remarkable memories recorded of certain feeble-minded people may really be due to an automatic repetition of perception such as we seem to have in some cases of the eidetic image? There is no reason why such a repetition, if it occurs, should be limited to the sense of sight.

Conclusions.

Visual images may be in perceived space (eidetic) or not. In both cases they have a true visual character. The eidetic image may or may not be formed around or with the aid of sense data. (The difference corresponds to the difference between illusion and hallucination.) The inner visual image makes no use of sense data, which indeed tend to hinder its formation. More evidence is required before we can determine the function of the eidetic image; and also before we can accept the theory that it is a phenomenon peculiar to child psychology. The mode of formation of images (automatic or spontaneous, as in dreams, on the one hand, as contrasted with volitional on the other) is possibly of greater significance in connection with primitive forms of memory than is their localisation.

(*Manuscript received* 16 *March* 1926.)

THE CONSTANCY OF THE INTELLIGENCE QUOTIENT—FINAL RESULTS

By P. L. GRAY and R. E. MARSDEN.

1. *Object of the investigation* (pp. 20, 21).
2. *The number of children re-tested* (p. 21).
3. *Change of I.Q.* (pp. 21–23).
4. *Children tested in* 1920 *and* 1925 (p. 23).
5. *Correction of the scale at the upper end* (pp. 23, 24).
6. *Diagnostic value of the tests* (p. 24).
7. *Consideration of special cases* (pp. 25, 26).
8. *Conclusions* (p. 26).

1. *Object of the Investigation.*

In 1920 we used the Stanford-Binet tests, as described by Terman in his *The Measurement of Intelligence*, to test 224 children in the elementary schools of a large city in the North of England. The tests were used exactly as printed in the record booklet for English users[1], except that one or two unavoidable alterations were made to conform to English usage; 'pence' was substituted for 'cents' for example.

Having satisfied ourselves that the tests gave valuable results when applied to English children we next determined to investigate whether the I.Q. as obtained by the testing would be constant if the tests were applied at yearly intervals for a number of years.

The I.Q. does not measure 'intelligence' in absolute units but merely indicates the position of a child in his age-group. It is everywhere recognised that the intelligence grows during childhood, so that to keep the I.Q. constant it is necessary for a child's intelligence to grow at a rate proportionate to his I.Q. If the growth of the intelligence of a normal child is assumed to have a normal rate then the intelligence of a child of I.Q. 150 must grow one and a half times as fast.

The earlier results of our investigation have been printed in this *Journal*[2], so that this paper will deal only with the final results. As the children leave the elementary schools, either to go to the secondary schools at about the age of eleven years, or to go to work shortly after they attain the age of fourteen, most of the children we originally tested had left the elementary schools in which all our testing was done by the time our investigation was completed.

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[1] Published by Messrs Harrap and Co.

[2] This *Journal*, XIII, pt. 3, Jan. 1923, and XV, pt. 2, Oct. 1924.

2. *The Number of Children re-tested.*

The results now presented are based on re-testing 100 children on the first anniversary of the first testing; 63 on the second; 57 on the third; 37 on the fourth and 23 on the fifth.

A few of the children were not tested every successive year as some happened to be away from school when the test was due and so missed being tested till the next anniversary came round.

The total number of children concerned was 109, made up of 44 children tested twice; 12 tested three times; 23 tested four times; 12 tested five times and 18 six times. The intervals between the tests compared were thus one, two, three, four or five years.

By comparing each I.Q. with all the previous ones for the same child a total of 616 comparisons is available for this paper.

3. *Change of I.Q.*

In Table I are shown changes of I.Q. for all children tested after intervals of one, two, three, four or five years. These have been grouped according to (1) the natural age at the earlier testing, (2) the I.Q.'s at the earlier testing, (3) the mental age at the earlier testing, (4) the intervals between the testings, and (5) sex.

When the whole of the 616 comparisons are examined it is found that:

(1) The central tendency of the change in terms of I.Q. is − 1·3 points.

(2) The middle 50 per cent. of changes lies between the limits of 6·1 decrease and 4·7 increase.

(3) The probable error (semi-inter-quartile range) of a prediction based on the earlier test is 5·5 points.

(4) The coefficient of correlation for the whole of the 616 comparisons is ·851 ± ·008. For the 100 comparisons dealt with at the end of the first re-testing the coefficient of correlation was ·883 ± ·015; for 371 comparisons at the end of the fourth testing it was ·854 ± ·011.

These figures show a slightly diminishing correlation as the interval between the testings increases. As we shall see, this may be due to some extent to the later tests being too difficult. It may also be partly due to the growth of intelligence ceasing at different ages in different persons;

P16299

TABLE I. *Showing I.Q. changes for children tested after intervals of one, two, three, four and five years, for children of different natural and mental ages, sex and various degrees of brightness.*

	N.A. at earlier testing				I.Q. group					M.A.			Interval					Sex		
Change of I.Q.	3 to 6 years	6 to 9 years	9 to 12 years	Above 12 years	Above 110	90 to 110	Below 90	Above 120	Below 80	4 to 8 years	8 to 11 years	Above 11 years	1 year	2 years	3 years	4 years	5 years	Boys	Girls	Total
More than 20	3	—	1	—	2	2	—	—	—	2	2	—	2	1	1	—	—	2	2	4
16 to 20	1	3	8	1	4	7	2	1	—	4	5	4	3	5	3	1	1	5	8	13
11 to 15	—	11	14	7	9	21	2	3	—	9	11	12	10	9	9	4	—	17	15	32
6 to 10	2	26	39	9	11	44	21	4	2	24	36	16	39	23	10	3	1	39	37	76
1 to 5	1	44	69	18	17	91	24	7	3	31	66	35	65	32	20	12	3	58	74	132
0	2	5	26	7	5	29	6	2	1	7	20	13	21	11	4	3	1	20	20	40
− 1 to − 5	7	59	77	20	30	108	25	11	2	34	88	41	82	42	23	11	5	80	83	163
− 6 to −10	2	51	32	11	18	61	17	9	5	23	63	20	30	27	21	12	6	43	53	96
−11 to −15	2	21	13	3	16	14	9	6	6	7	23	9	10	11	9	7	2	13	26	39
−16 to −20	5	10	2	—	13	3	1	7	2	8	9	—	1	3	6	4	3	1	16	17
More than −20	2	2	—	—	3	—	1	—	—	3	1	—	—	1	1	1	1	—	4	4
Totals	27	232	281	76	128	380	108	50	21	152	314	150	263	165	107	58	23	278	338	616
Median change	−2·8	−1·5	0	0	−2·9	−0·7	−0·6	−5·0	—	−0·7	−1·6	−0·8	0	−1·1	−3·3	−3·0	−6·3	−0·8	−1·6	−1·8
Semi-inter-quartile range	10	5·6	5·8	4·8	7·5	5·0	5·4	6·6	—	6·8	4·8	4·6	4·6	5·6	5·7	5·6	6·4	5·1	5·6	5·4

the facts point to the early cessation of growth in persons of low intelligence.

Comparable figures obtained by other investigators were given in one of our previous papers[1].

4. *Children tested in* 1920 *and* 1925.

As it was not possible to test all of the 23 children tested in 1925 in each year between that date and the original testing, all those children have not been tested six times, but it happens that all were tested in the first, fourth, fifth and sixth years of our investigation. The average I.Q. for these 23 children in those years was 102·1, 97·0, 98·0 and 96·6. A decrease of I.Q. is shown by 19 children, one shows an increase of 17 points in five years and three show an increase of 3 points in the same time. At one time it was thought that practice year by year would probably cause an increase in the I.Q. obtained with the same scale of tests. Workers with the tests are often surprised to find how little is remembered of the tests from year to year. As the children are never told whether their replies to the questions are right or wrong the same mistake is sometimes made several years in succession; it is particularly noticeable how much alike are the answers to the Ball and Field test in successive years given by the same children.

The general decrease in I.Q. shown above may be due either (1) to the too great difficulty of the tests at the upper end of the scale or (2) to a general lowering of the intelligence. As the four cases of increase are made by children with I.Q.'s between 91 and 102, while the decreases take place in I.Q.'s ranging between 80 and 138, it can hardly be due entirely to the lowering of intelligence.

5. *Correction of the Scale at the Upper End.*

The inadequacy of the tests in the upper end of the scale is acknowledged by Terman in his *Mental and Physical Traits of a Thousand Gifted Children*. The correction used in the investigations detailed in that book is:

Tests passed in XVI and XVIII	5	6	7	8	9	10	11	12
Number of months to be added to mental age	3	6	9	12	15	18	21	24

These figures have not been obtained by the re-standardisation of the tests but by making certain assumptions based on a general experience of testing. The 'corrections' have been applied to all the children in our investigation who have answered sufficient tests in XIV and XVIII successfully, and the corrections appear to be too generous. The small number of our children to which the corrections can be applied makes it unsound to attempt alternative figures.

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[1] This *Journal*, xv, pt. 2, Oct. 1924.

Dr Burt has also found these tests at the top end of the scale too difficult for English children and is re-standardising the scale. The group of American children on whom they were tried was well above the average of both American and English children.

As we pointed out in our first paper another thing that contributes to differences in I.Q.'s obtained by the tests is that at the upper end of the scale a child may just earn or just fail to earn as many as six months on a single test; this is too much to depend on 'score or fail' in any one test. Scores for partially correct answers would often render the successive I.Q.'s for a child nearer to each other.

In every case the teachers' knowledge of the children and their progress in school were compared with the results of the tests. Our experience leads us to think that as a whole the tests as they stand serve their purpose well, but that improvements would probably be made if (1) the tests, especially at the upper end, were re-standardised for English children, (2) the scale were extended at its upper end and tests inserted for XV and XVII, (3) some tests were omitted.

Instead of the discarded tests it might be possible to include some of Burt's reasoning tests, some of Ballard's harder absurdities, Porteus mazes and other non-literary tests and, perhaps, some of the more trustworthy simple performance tests.

In re-standardising the tests care should be taken to use a truly representative group of testees including all social grades, degrees of brightness and age.

6. *Diagnostic Value of the Tests.*

The diagnostic value of the tests as they stand is high as is shown by the table of deviations:

TABLE II. *Deviations of I.Q.* 616 *Comparisons.*

Deviations as great as or greater than	Percentages	
	616 comparisons	Theoretical
5·5	50·5	50·0
11	17·8	17·7
16·5	5·2	4·3
22·0	0·9	0·7

7. *Consideration of Special Cases.*

In this section will be briefly considered children among those tested at the last testing whose series of I.Q.'s show variations as much as 10 points. After the initials of the child its natural age is given, in years and months at last testing, and then the successive I.Q.'s. A blank indicates that the child missed being tested in the year for which the blank stands.

There are several cases in which a steady fall of I.Q. is indicated.

	Age		I.Q.					
	yrs.	mths.						
C.T.	13	4	80	89	90	86	79	74
A.F.	12	6	88	86	82	79	73	75
E.N.	11	2	100	—	—	95	83	83
M.W.	13	5	101	93	88	88	81	80
F.T.	11	2	100	107	106	98	98	97
M.E.	10	11	115	113	101	92	93	97
D.B.	10	1	134	134	132	120	116	117

The apparent fall in these cases may be partly due to an increasing difficulty of the tests at the upper end of the scale and partly to a gradually decreasing rate of growth of the intelligence; the growth of intelligence probably slows down before stopping. It seems natural to suppose that if the intelligence stops growing at different ages in different children then it stops growing earlier in children of low intelligence. There appears to be no way of detecting how much of the drop may be due to each of the causes mentioned till the scale has been re-standardised.

The child with I.Q. 134 is an unusual case of a high I.Q. falling; Terman's classification of a very superior child is one whose I.Q. lies between 120 and 140. Nearly all this child's I.Q.'s are between those two limits.

The next is a very exceptional case of almost steady increase:

	Age		I.Q.					
	yrs.	mths.						
B.B.	11	6	100	104	100	107	111	117

The rest are irregular:

D.M.	12	2	97	104	98	97	90	93
J.B.	13	0	106	105	100	93	100	100
K.H.	12	11	110	—	97	101	109	102
K.G.	10	3	104	110	110	97	102	99
J.A.	11	10	102	112	—	97	104	105
A.P.	11	4	103	108	95	92	106	102

The method of scoring by which a child either passes or fails, and no allowance is made for part scores, explains some of the irregular figures found above. Even though the variation is as much as 10 points it will be seen that by assuming the true I.Q. to be plus or minus five different from that given, no great injustice would be done to the children concerned. After all the I.Q. is only an attempted measure of the intelligence of the child; school work should prove the real worth of the figure obtained.

L.P. 13 yrs. 8 mths. 138, 132, 130, 130, 133, 126.

Terman's correction applied here would make the last I.Q. 131. This girl had a splendid vocabulary of 72 words and so just failed, wanting four more words, to score at XVIII (Superior Adult).

When it is remembered that the other 11 children tested in the years from 1920 to 1925 show no difference of range as great as 10 points it will be seen how close are the successive I.Q.'s, and when it is also remembered that the testing was done by two different people almost alternately the agreement is all the more remarkable. The children who were tested for the last time in years previous to 1925 are dealt with in our earlier papers.

8. *Conclusions.*

(*a*) In their present form the Stanford revision of the Binet tests has a high diagnostic value as a whole.

(*b*) There seems to be no very marked difference of variability or of median change in I.Q. (Natural age, I.Q., mental age, interval between the tests, and sex, all appear to have no special influence on the change of I.Q.)

(*c*) The probability of an I.Q. showing an increase or a decrease of as much as 5·5 points is one in two; as much as 11 points, one in five; as much as 16·5, one in twenty, and for as much as 22 the chances are one in a hundred. In other words the probability is that 80 times out of 100 the I.Q. change will not exceed 11 points; in 95 not exceed 16·5 points, and 99 times out of 100 will not exceed 22 points; this shows the high prognostic value of the tests.

(*d*) Practice once a year appears to have very little effect on the I.Q. as measured by the Stanford-Binet tests.

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A COMPARISON OF GROUP VERBAL AND PICTORIAL TESTS OF INTELLIGENCE[1]

By CONSTANCE M. DAVEY.

1. *Introduction* (pp. 27–30):
    (*a*) *The need for non-verbal tests* (pp. 27, 28).
    (*b*) *Conflict of opinion as to the respective functions of verbal and non-verbal tests* (p. 28).
    (*c*) *The problems arising from this conflict of opinion* (pp. 28, 29).
    (*d*) *Experimental results of other investigations* (pp. 29, 30).
    (*e*) *The aim of the present investigation* (p. 30).
2. *Preliminary experiments* (pp. 30–34).
3. *The main experiment* (pp. 34–37):
    (*a*) *Its problem* (p. 34).
    (*b*) *Procedure* (pp. 34–36).
    (*c*) *The statistical results* (pp. 36, 37).
4. *Examination of the results of the main experiment* (pp. 37–46):
    (*a*) *The problem of the positive bias* (pp. 37, 38).
    (*b*) *Search for 'group factors'* (pp. 38–45).
        (1) *Group factors intrinsic to the test* (pp. 38–42).
        (2) *Group factors extrinsic to the test* (pp. 42–45).
      The result of eliminating 'group factors' (pp. 45, 46).
5. *Summary of conclusions* (pp. 47, 48).

1. INTRODUCTION.

THE general criticism passed upon many verbal tests, says Prof. Terman, is that they depend chiefly on a language factor, and for this reason are considered invalid as measures of 'intelligence.' The upholders of such an opinion would substitute for these tests some which use non-verbal material, for by so doing they claim to provide a truer measure of 'intelligence.'

(*a*) *The need for non-verbal tests.*

It was not with this end in view, however, that the non-verbal test was devised. It was with a more practical problem American psychologists were confronted, a more immediate need, definitely stated by Brigham when he wrote as follows:

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[1] From a thesis approved by the University of London for the degree of Doctor of Philosophy.

The dearth of intelligence tests that are independent of the language factor indicates the magnitude of the problem that faces American investigators, who wish to test the intelligence of individuals who have not had adequate training in English—immigrants, children of non-English speaking parents, etc.[1]

Non-verbal tests were therefore devised in order to meet this immediate need of measuring the intelligence of certain individuals: they did not originate in the opinion that the verbal test was inadequate because it measured 'linguistic ability' rather than 'intelligence.' It is since the use of these tests has become general that the criticism as to the validity of substituting a non-verbal for a verbal test has arisen.

(b) *Conflict of opinion as to the respective functions of verbal and non-verbal tests.*

The need of the moment having been met, and such tests having been made available for general use, divergent opinions as to the relative value of these different types of test as measures of 'intelligence' arose among investigators. We find Prof. Thorndike maintaining that "the ability measured by verbal tests is not the same as the ability measured by non-verbal tests[2]." On the other hand, there are those who would hold with Prof. Spearman that all mental performances can be in part attributed to one general common function, and that this general factor 'g' is what both the verbal and the non-verbal tests should measure. It is to the solution of the problem raised by such conflicting views that we have directed our experiments in the present investigation.

(c) *The problems arising from this conflict of opinion.*

First let us state the various alternative solutions of the problem as clearly as possible.

The main question at issue is whether these two types of test, the verbal and the non-verbal, are testing one and the same function. An answer in the negative may imply:

either (1) that both measure absolutely different things, neither of which may be classed as 'general intelligence,'
or (2) that only one of these types of test measures something which falls within the category of 'intelligence,'
or (3) that though both measure something different, yet they are each parts of a larger whole: 'intelligence.'

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[1] *Psych. Rev.* Monog. vol. xxiv, p. 236.
[2] Thorndike, *Journ. Educ. Psych.* 1921.

An answer in the affirmative may mean:

either (4) that the two types measure only the function common to both,
or (5) that both measure the same common factor, but that each measures as well some specific factor, in equal degrees,
or (6) that one of these types alone measures a specific factor in addition to the general factor common to both.

If any one of these affirmative solutions be correct, then we have in addition to answer the question whether both types of test, while measuring the same thing, measure it with equal accuracy.

The reason for the great variety of opinion already pointed out now becomes more evident, for the answer to our question is not one of mere affirmation or negation.

Having thus analysed the problem involved in this conflict of opinion, we now turn to consider more fully that aspect of it which has received most emphasis. That is, whether solution (6) be the true one, in that the verbal test, besides measuring a common factor which may be called 'intelligence' also measures to some appreciable degree a specific factor, 'linguistic ability.'

(*d*) *Experimental results of other investigations.*

A comparison has been made between verbal and non-verbal tests by W. T. Root, who compares their relative value as measures of intelligence when correlated with the Binet scale. He finds the correlation of verbal tests (as represented by the mentimeter) with the Binet scale to be + ·88, while the non-verbal (Dearborn test) is + ·87[1]. He therefore concludes (on the assumption that the Binet standard is correct) that both are equally good measures of 'intelligence.' This hardly agrees with the work of A. L. Gates, who discusses the relative value of verbal and non-verbal tests for predicting success in school subjects. Mr Gates incidentally approaches our problem, for he makes a comparison between the results of verbal and non-verbal group tests of intelligence. Unfortunately, all the tests could not be applied to all the children tested, but "Grade III offers an opportunity to test the relative validity of verbal and non-verbal tests for the reason that there are available a number of tests constructed exclusively of each type of material[2]." It is on the correlations[1] obtained from the results of this grade of "about twenty children" for seven non-verbal, five verbal and several scholastic tests that Mr Gates bases the following conclusions: "The verbal (tests) are more consistent with themselves than the non-verbal, but the latter agree better with themselves than with the verbal." "The verbal tests clearly yield different results from the non-verbal." The high correlation between the verbal tests, we suggest, may be due to a 'group factor' in common to these tests when they are compared with tests non-verbal in type. Whether this 'group factor' can be allocated to the verbality of these tests we shall have to consider later. Our point here is, that the validity of such a conclusion is rendered suspicious by the fact that these different tests are not independent of each other.

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[1] *Journ. Educ. Psych.* 1922, pp. 286–292.

[2] *Journ, Educ. Psych.* 1922, p. 129.

Another investigator, A. M. Jordan, correlates the individual tests of the Otis, Miller, Alpha and Terman group tests with the Stanford-Binet mental age of his subjects in order to discover which of the four group tests is the best measure of 'intelligence.' He thus secures to some extent the independence of his units of measurement, but not entirely so, for he does not correlate the individual tests among themselves. We, therefore, feel that further enquiry is still necessary in order to detect or eliminate factors which tend to distort the perception of the functions fulfilled by these various types of test.

(e) *The aim of the present investigation.*

In the following pages we attempt to carry such investigations a step further; firstly by devising a pictorial test, which shall closely parallel a verbal test already standardised; and secondly by submitting to careful statistical examination the correlations obtained between each individual test and all the others, both of its own type and of the contrasted type. By this means we hope to arrive at a clearer understanding of the relative functions of oral and pictorial tests of 'intelligence.'

2. PRELIMINARY EXPERIMENTS.

In order to carry out the investigation, a test of intelligence purely verbal in character was necessary. This test was supplied by Prof. Spearman, who kindly allowed me to use an oral group test arranged and standardised by himself.

The next step was the planning and devising of a pictorial test on the same lines as that of the oral test. In order to eliminate as many irrelevant factors as possible, it seemed advisable to make the procedures used in giving the tests as identical as one could. The instructions were, therefore, drawn up in a similar manner. Thus, in both tests the instructions were given orally, and the answers written in a similar manner. The next problem that arose was how far the tests could be made to correspond in form and content. It was possible to devise 'classification,' 'analogies,' 'questions,' and 'completion' tests to correspond in form, but how far could the actual words used in the oral test be replaced by pictures? This was considered only practical in the use of concrete substantives, for as soon as the words became abstract or attributive in character, the pictorial test became too artificial. The difficulty becomes obvious on attempting to form a pictorial classification test to correspond with the oral one:

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[1] These average correlations are: Inter-verbal ·62, Inter-non-verbal ·40, Between verbal and non-verbal ·24.

rash, wild, imprudent, happy.

Pictorial tests on all these and other lines formed the material for two preliminary experiments. These were necessary for two reasons: first, to test the tests themselves; and secondly, to determine which pictorial tests should be used in the main experiment.

In the first experiment the tests were tried out on the following six groups of girls:

(1) 10 girls, age 13 years, considered dull by their teachers;
(2) 8 girls, age 11, considered bright;
(3) 9 girls, age 11, considered dull;
(4) 9 girls, age 9, considered bright;
(5) 9 girls, age 9, considered dull;
(6) 9 girls, age 7, considered bright;
making a total of 54 subjects.

After a careful examination of the results, trying especially to find out whether the errors made were due to the test itself, and to see whether the tests did separate the dull from the bright group of the same age, a corrected set of tests was formed as material for the second experiment. This was conducted in the same school, being given to six similar groups of 52 girls, none of whom had been in the first set of groups.

At the outset it was necessary that each test of the preliminary experiments should distinguish the dull from the bright groups, and on that account we rejected the spatial tests, for these showed themselves to depend for their solution on some specific factor besides the general factor with which we are chiefly concerned. The picture interpretation test had to be omitted on the ground of the difficulty of its marking; and in order to make the length of time taken by oral and pictorial tests about the same, we had to reject several of the latter which had no exact correspondence in form to the former.

Our final selection of pictorial tests then for the main experiment was as follows:

(1) *Classification test.*

In the oral test of this name four words were given, three of which bore some relation of similarity to each other; the fourth being unlike the rest. Our pictorial test was made to correspond with it as closely as possible, four pictures being substituted where practical for the actual words used. This could only be done when the words were not abstract, *e.g.*

(*a*) trumpet, violin, table, drum.
(*b*) book, newspaper, letter, grapes.

(2) *Analogies test.*

In the planning of the analogies test, it was again found impossible to adhere very closely to the words used in the oral test, mainly on account of their abstract nature. The test itself was more difficult to give pictorially, in respect of making of directions clear and simple enough for children to follow. In the first preliminary experiment the subjects were given a short practice oral test, and then did the picture tests, but this method proved unsatisfactory, and in the second experiment the first two questions of the pictorial analogies were worked with them. There certainly appeared to be less confusion on the part of these children, if we judge from the manner in which they attacked the rest of the test.

In examining this test qualitatively we find that its solution depends on the perceiving of a relation between the first two terms and the finding of a correlate to the third term which bears the same relation to it as that already educed between the first and second terms[1]. This correlate had to be selected from four pictures. The younger children found this test difficult. It was not till we examined the results of the eleven-year-old group that we found a significant difference between the dull and bright groups.

(3) *Sequence test.*

This test was similar in type to one of the Beta Army Tests. A series of four pictures told a story; the pictures, however, were not separate but fixed in a given position on a card. The subject was asked to write the numbers given under the pictures in such an order that the pictures should 'tell a story.'

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[1] C. Spearman, *Nature of Intelligence and Principles of Cognition*, p. 80.

The solution of the test depended chiefly upon the educing of the causal relation, which, judging from these results, appears to be more difficult of eduction for the younger children than does the relation of similarity. With the exception of some of the dull nine-year-old children, all our subjects found correctly the first picture of the series. The dull groups, however, get little further; they simply write the numbers in the order they appear on the card.

The test was successful in the distinguishing of bright from dull subjects: its coefficient of colligation being ·50 with a P.E. of ·046. We, therefore, added two other picture series to this series for our main experiment.

(4) *Completion test.*

This test involved the reading of a story in which gaps were left, and these gaps had to be filled in so as to accord with a given picture. This test, therefore, was not a purely pictorial one, although it differed from the oral completion test, for there the sentence to be completed was given orally, as were also the four words from which the answer had to be selected. Any appreciable difference due to this addition of reading matter to the test will be discussed later (p. 41).

The dull groups seemed to find difficulty in retaining what had been read when they turned to the picture, or in remembering the details of the picture when they turned from it to the reading material, and a tendency to fill in gaps without reference to the picture was frequent.

(5) *Questions test.*

This test was planned to correspond with the oral test of that name, and differed from it mainly in that the answer had to be selected from a picture instead of from four words. The questions of this test were given orally, the picture being "When did you last see your father?" The title had been removed. The test was not, therefore, purely pictorial, a point to be considered later.

(6) *Enumeration test.*

In this test the subjects were asked to write as quickly as they could everything they could see in the picture. The time allowed for them to do this was three minutes. The dull groups, it was noticed, had finished before the time expired; the speed with which they wrote was not, therefore, a factor in their results, though it may still be so in the results of the bright groups.

Prominent details were named first, and the question here seems to be whether the dull get beyond the first stage. The test would appear then to consist in "mere clarification of items given originally," and not in new knowledge being generated. In the preliminary experiments this test separated the dull from the bright and it was, therefore, included in our main experiment.

Subsequent experiment, however, tended to show that it is a doubtful measure of the degrees of 'general intelligence.'

3. THE MAIN EXPERIMENT.

(a) *Its problem.*

The main problem of this investigation was the relation between oral and pictorial tests of intelligence. Does this difference in the material used make any appreciable difference in the results? That is, for practicable purposes, can a pictorial test of general intelligence be substituted for an oral one?

The means taken for the solution of this problem was the quantitative examination of the results obtained in the following experiment.

(b) *Procedure.*

(1) *Subjects.*

The finally selected oral and pictorial tests were given to children in the L.C.C. schools. In all, 243 children were tested, and of these, six groups were in different classes of different schools. The seventh group, on the other hand, was composed of 82 boys, all of whom belonged to the same school; this group was divided into four sub-groups according to their age. The ages of the children tested ranged from 8 years to 14 years.

(2) *Statistical methods used.*

In the oral group test one mark was given for each correct answer, and this method was adhered to for the pictorial group test. The total marks obtained for each individual test of both types were then ranked for the separate groups.

The correlations of the fourteen tests, *i.e.* eight oral and six pictorial, were then calculated for each of the ten groups, the formula used being

$$r = 1 - \frac{6Sd^2}{n(n^2-1)},$$

where n = the number of children in each group[1]. We then proceeded to use the results of the 91 inter-correlations obtained for the eight oral and six pictorial tests. These correlations for the ten groups were then averaged; the results so obtained are given in Table A (p. 36).

The advance made recently in giving to psychology more adequate statistical methods, brings with it the possibility of applying definite formulae to these inter-correlational tables. By their use we are now able to estimate the factor of 'general intelligence' in different types of tests, and, in addition, to detect the influence which other factors, common to two or more but not to all the tests, may have upon the correlation coefficients obtained experimentally.

The formula we employed here was that of proportionality as announced in the comparatively early writings[2] and proved later to be derivable directly from Yule's partial correlations[3]. This formula $\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}}$[4] has now been made available for actual use, its probable error having recently been calculated[5]. We are thus enabled to apply the proportional formula to the oral and pictorial tests, when its form becomes:

$$\frac{r_{o_1 o_2}}{r_{o_1 p_2}} = \frac{r_{o_1 p_2}}{r_{p_1 p_2}}, \text{ where } o_1 o_2 = \text{oral tests, } p_1 p_2 = \text{pictorial tests.}$$

By so doing, we claim that we are able to show whether the pictorial test measures only the same 'general intelligence' as does the oral test. For if the two oral tests o_1, o_2 should involve a group factor due to their both being verbal, the value of $r_{o_1 o_2}$ will measure, besides the common factor 'g,' this group factor, and therefore its value will be too high to fit the above equation. Similarly, should a group factor be found between p_1 and p_2, due to their pictoriality, $r_{p_1 p_2}$ would also be too big to fit the equation. Then, as neither of these group factors will affect the values of $r_{o_1 p_2}$ and $r_{p_1 o_2}$, we should get

$$\frac{r_{o_1 o_2}}{r_{o_1 p_2}} > \frac{r_{o_1 p_2}}{r_{p_1 p_2}}.$$

But, on the other hand, should the above two values be approximately equal, we can legitimately conclude that no such 'group' factor is involved, and that both types of test have measured one and the same general factor, and are, therefore, equally applicable as tests of 'general intelligence.'

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[1] C. Spearman, this *Journal*, II, p. 98.

[2] *Amer. Journ. Psych.* 1904.

[3] This *Journal*, 1912, v, 58.

[4] *Introduction to Statistics*, p. 235.

[5] Communicated by Spearman and Holzinger at meeting of Brit. Psych Soc., Feb. 1924. Published, this *Journal*, July 1924, p. 19. *I.e.* where $F = r_{o_1 o_2} r_{p_1 p_2} - r_{o_1 p_2} r_{p_1 o_2}$ and r = average value, then $\text{P.E.}_F = 1{\cdot}349 \sqrt{\frac{r^2 (1-r)^2}{n} + \frac{(1-r^2)^4}{n^2}}$.

The formula was thrown into the more convenient form

$$r_{o_1 o_2} r_{p_1 p_2} - r_{o_1 p_2} r_{p_1 o_2} = 0,$$

and each pair of the oral tests was taken with each pair of the pictorial tests, *i.e.* 28 pairs of oral tests were used severally to form combinations with 15 pairs of pictorial tests. The 420 results so obtained were then graphed (Table B) and the probable error of the left side of the above equation calculated.

(*c*) *The statistical results.*

We turn now to the actual results obtained in this investigation, and give first the inter-correlational table for the individual tests.

TABLE A. *Inter-correlations.*

	Oral tests								*Pictorial tests*					
Oral tests	1	2	3	4	5	6	7	8	I	II	III	IV	V	VI
Same and opposite 1		·47	·60	·51	·42	·32	·34	·33	·32	·29	·34	·33	·34	·39
Synonyms 2	·47		·61	·47	·44	·38	·37	·42	·35	·39	·40	·46	·40	·32
Classification 3	·60	·61		·61	·49	·48	·41	·42	·26	·36	·39	·48	·35	·43
Questions 4	·51	·47	·61		·45	·36	·31	·40	·23	·23	·36	·39	·39	·36
Completion 5	·42	·44	·49	·45		·35	·30	·30	·29	·33	·33	·30	·38	·32
Analogies 6	32	·38	·48	·36	·35		·29	·31	·23	·26	·30	·41	·30	·24
Inference 7	·34	·37	·41	·31	·30	·29		·23	·27	·38	·33	·28	·26	·16
Likeliness 8	·33	·42	·42	·40	·30	·31	·23		·25	·33	·25	·22	·31	·25
Pictorial tests														
Classification I	·32	·35	·28	·23	·20	·27	·27	·25		·32	·33	·29	·25	·35
Analogies II	·29	·39	·30	·23	·33	·26	·38	·33	·32		·29	·38	·28	·20
Sequence III	·34	·40	·39	·36	·33	·30	·33	·26	·33	·29		·35	·32	·16
Completion IV	·33	·46	·48	·39	39	·41	·28	·22	·29	·38	·35		·36	·33
Questions V	·34	·40	·35	·39	38	·30	·26	·31	·25	·28	·32	·36		·38
Enumeration VI	·39	·32	·43	·36	32	·24	·16	25	·35	·20	·16	·33	·38	

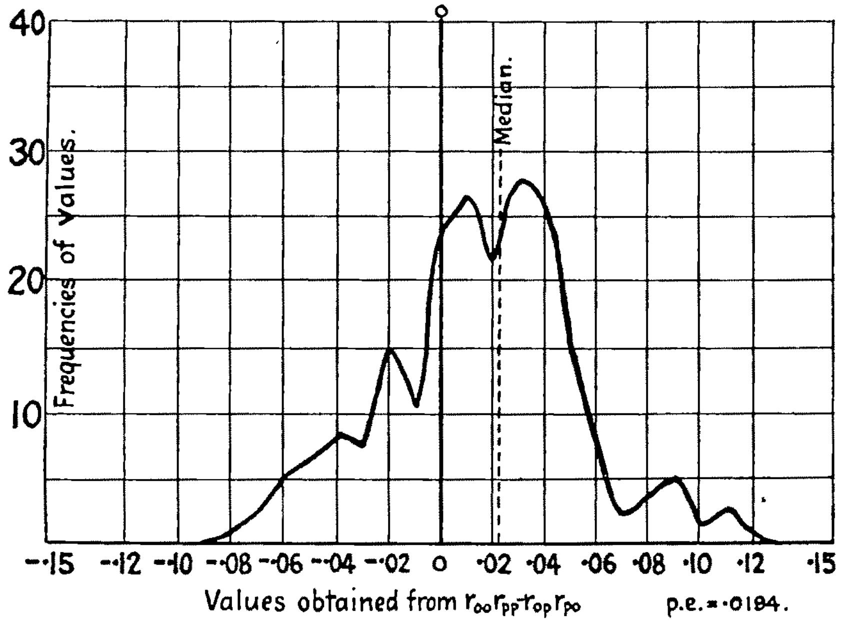
Average correlations:
for whole table $\bar{r} = \cdot 34$ P.E. $= \cdot 039$
for oral tests $\bar{r} = \cdot 40$ P.E. $= \cdot 037$
for pictorial tests $\bar{r} = \cdot 31$ P.E. $= \cdot 040$
for oral-pictorial tests $\bar{r} = \cdot 32$ P.E. $= \cdot 040$
$n = 243$ subjects.

The application of the formula $r_{o_1 o_2} r_{p_1 p_2} - r_{o_1 p_2} r_{p_1 o_2}$ to the inter-correlations given in Table A is thrown into the graphical form below (Table B).

Now, if the correlations used in compiling this graph were measures of the 'general factor' only, we should find, firstly, that its average value approximates to zero, and secondly, that its median dispersion is about equal to the probable error of $r_{o_1 o_2} r_{p_1 p_2} - r_{o_1 p_2} r_{p_1 o_2}$, *i.e.* ·0194. These two conditions our observed correlations must satisfy, if we are to claim that both types of test measure only on the same 'general intelligence.'

On examining our graph, what we actually find is, that the median falls at + ·021, and that 208 of the 420 results fall within the median plus or minus the error (·0194) due to sampling. That is, the dispersion is very closely that which we should expect it to be from mere sampling, but the median is shifted from zero to + ·02.

TABLE B.



One of the required conditions has, therefore, been fulfilled, that is, the median dispersion of the curve is such as our sampling error would lead us to expect, but, on the other hand, our average value has not tended to zero but to + ·02. This, as an inspection of our graph shows, although very small, is still more than could be expected from chance.

4. EXAMINATION OF THE RESULTS OF THE MAIN EXPERIMENT.

(*a*) *The problem of positive bias.*

The problem that now arises is that of ascertaining the cause of this bias. If our observed correlations had been measures of the general factor only, we should have found that our results on the average fitted the equation:

$$\frac{r_{o_1 o_2}}{r_{o_1 p_2}} = \frac{r_{o_1 p_2}}{r_{p_1 p_2}}; \text{ or that } r_{o_1 o_1} \cdot r_{p_2 p_2} - r_{o_1 p_2} \cdot r_{p_1 o_2}$$

approximated to zero. But, as we have shown, our observed correlations give a positive bias to our curve, and we are therefore forced to conclude that another factor, common to some of the tests but not to all, has influenced our results. This factor may be common to several or all of the verbal tests, though not found in the pictorial tests; or it may be common to several or all of the pictorial tests, but not involved in the oral tests. Such factors have been called 'group factors[1].'

Of these two alternatives, the former, *i.e.* a group factor common to the verbal tests, seems the more likely. But, even if this be so, there still remains an important question to ask. Does such a group factor exist *throughout* the verbal tests, or is it common to several but not to all of these tests? To find that it exists *throughout* all the verbal tests, and at the same time is not to be found in the pictorial tests, will naturally mean that verbality as such must be regarded as a group factor that influences these verbal tests as tests of 'general intelligence.'

On the other hand, a group factor common to *several* of the verbal tests, but not existing throughout *all* of them, must point to the conclusion that verbality itself is not the source of this group factor.

(*b*) *Search for 'group factors.'*

(1) *Group factors intrinsic to the test.*

We consider first certain possible group factors, which, because their source is the test itself, may be termed intrinsic. The perusal of the results of $r_{o_1 o_2} r_{p_1 p_2} - r_{o_1 p_2} r_{o_2 p_1}$ shows that only eleven of the results so obtained are greater than five times the probable error[2], and these eleven results, therefore, must be taken to lie outside the range of chance. They are composed of the following groups:

1. $r_{o_1 o_3} r_{p_1 p_2} - r_{o_1 p_2} r_{p_1 o_3} = \cdot 1096$ P.E. $= \cdot 021$.
2. $r_{o_1 o_3} r_{p_2 p_4} - r_{o_1 p_4} r_{p_2 o_3} = \cdot 1092$ P.E. $= \cdot 025$.
3. $r_{o_1 o_4} r_{p_1 p_5} - r_{o_1 p_5} r_{p_1 o_4} = \cdot 1003$ P.E. $= \cdot 020$.
4. $r_{o_2 o_3} r_{p_1 p_5} - r_{o_2 p_5} r_{p_1 o_3} = \cdot 1095$ P.E. $= \cdot 024$.
5. $r_{o_1 o_2} r_{p_1 p_3} - r_{o_1 p_3} r_{p_1 o_2} = \cdot 1166$ P.E. $= \cdot 020$.
6. $r_{o_1 o_3} r_{p_1 p_5} - r_{o_1 p_5} r_{p_1 o_3} = \cdot 1216$ P.E. $= \cdot 021$.
7. $r_{o_1 o_4} r_{p_2 p_4} - r_{o_1 p_4} r_{p_2 o_4} = \cdot 1179$ P.E. $= \cdot 020$.

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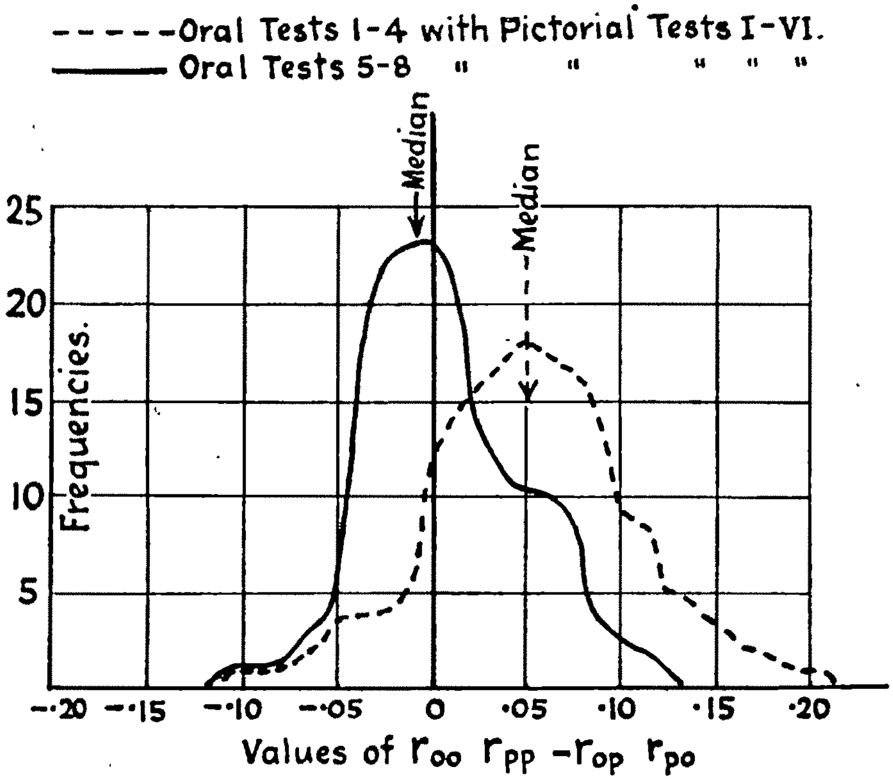
[1] *Amer. Journ. Psych.* XV.

[2] When the average for r is used, the P.E. $= \cdot 0194$.

8. $r_{o_2o_3}r_{p_5p_6} - r_{o_2p_6}r_{p_5o_3} = \cdot 1198$ P.E. $= \cdot 021$.
9. $r_{o_3o_4}r_{p_1p_5} - r_{o_3p_5}r_{p_1o_4} = \cdot 1330$ P.E. $= \cdot 021$:
10. $r_{o_3o_4}r_{p_3p_4} - r_{o_3p_4}r_{p_3o_4} = \cdot 1214$ P.E. $= \cdot 022$.
11. $r_{o_3o_4}r_{p_1p_2} - r_{o_3p_2}r_{p_1o_4} = \cdot 1116$ P.E. $= \cdot 021$;

These figures point to the possibility of some group factor in the first four oral tests, for we find that it is these tests only of the eight oral tests that are in the above list.

Showing group factor in oral tests 1–4.



We turn, therefore, to look for further evidence of such a factor.

(i) *The presence of a 'group factor' detected.* If there is, then, a group factor in the first four only of these oral tests, its presence will be revealed when the formula $r_{o_1o_2}r_{p_1p_2} - r_{o_1p_2}r_{p_1o_2}$ is applied to these four with all the pictorial tests; but it will not be revealed when this same formula is applied to the oral tests 5–8 with all the pictorial.

The graph above gives the results obtained. From it we see that the median of the curve when the oral tests 1–4 are used is at ·05, but that when we use the oral tests 5–8 the median falls at ·005. This points to the fact that a group factor is common to the first four oral tests, but does not extend in any marked degree to the oral tests as a whole.

Additional evidence for the presence of this group factor may be obtained from the oral tests themselves. For, if there is such a group factor common to the oral tests 1–4 only, its existence should be detected in the oral tests alone.

Taking tests 1–4 as $r_{o_1o_2}$, and tests 5–8 instead of $r_{p_1p_2}$, and applying our formula to the above correlations, we find that our median approximates ·01, and this we take corroborates the evidence we have already found for the presence of a group factor in the oral tests 1–4.

There is no such evidence, however, for the presence of a similar group factor among the pictorial tests. An examination of the results of the inter-pictorial correlations in Table A (p. 36) makes this fact obvious.

(ii) *The nature of this group factor discussed.* So far, then, we have shown that a factor exists between the first four oral tests, and we now proceed to ask whether its nature can be discovered. We have shown that its source cannot be verbality as such, for if this had been its origin the group factor would have shown itself throughout all the oral tests. Is it due then to the 'form' or 'content' of the test? By 'form' we mean such distinctions as 'classification,' 'analogies,' 'completion' and 'questions,' that occur in each type of test. If the 'form' is the source of this group factor, it will appear in the results obtained by the application of our proportionality formula. For, if the form 'classification' or 'analogies' contains a common group factor, we should find that the correlations from two such tests would be too high to fit into the proportional equation, and $\frac{r_{c_1c_2}}{r_{a_1c_2}}$ would be greater than $\frac{r_{c_1a_2}}{r_{a_1a_2}}$, where c_1 and c_2 are any two classification tests, whilst a_1 and a_2 are any two analogies tests.

Using all the relevant correlations in Table A (p. 36), we get:

1. $r_{c_1c_2}r_{a_1a_2} - r_{c_1a_2}r_{a_1c_2} = -\cdot0852$,
2. $r_{c_1c_2}r_{q_1q_2} - r_{c_1q_2}r_{q_1c_2} = -\cdot1121$,
3. $r_{c_1c_2}r_{m_1m_2} - r_{c_1m_2}r_{m_1c_2} = -\cdot0378$,
4. $r_{a_1a_2}r_{q_1q_2} - r_{a_1q_2}r_{q_1}r_{a_2} = +\cdot0207$ P.E. $= \cdot016$,
5. $r_{a_1a_2}r_{m_1m_2} - r_{a_1m_2}r_{m_1a_2} = -\cdot0046$,
6. $r_{m_1m_2}r_{p_1p_2} - r_{m_1p_2}r_{p_1m_2} = +\cdot0039$ P.E. $= \cdot020$,

where
c_1 = classification oral,
c_2 = classification pictorial,
a_1 = analogies oral,
a_2 = analogies pictorial,
q_1 = questions oral,
q_2 = questions pictorial,
m_1 = completion oral,
m_2 = completion pictorial.

All these results, we see, are too small to be significant, four of them being minus values.

The source of this group factor, then, in the first half of the oral test, does not appear to be due to their form in any appreciable degree, and must therefore be attributed to their content, that is, either to the fundaments, or to the similarity of the relations educed in the solution of the tests[1].

(iii) *The introduction of reading material into the pictorial test.* A problem, rather different in nature, arises from the pictorial tests themselves. These tests fall into three groups:

(*a*) purely pictorial,
(*b*) pictorial plus the reading of verbal matter,
(*c*) pictorial plus oral questions,

and the question at issue, here, is whether these differences give rise to differences in the results obtained. First, does the addition of the reading matter into the pictorial tests introduce a group factor in those tests, which will not be found in the purely pictorial tests? To answer this question, let us take any two purely pictorial tests p_1 and p_2, and two other pictorial tests c_1, c_2, which include reading matter. Then

$$r_{p_1 p_2} r_{c_1 c_2} - r_{p_1 c_2} r_{c_1 p_2} = 0,$$

if no such group factor exists.

Using the correlations found in Table A (p. 36), we get:

$$r_{p_1 p_2} r_{c_1 c_2} - r_{p_1 c_2} r_{c_1 p_2} = -\cdot 0036 \quad \text{P.E.} = \cdot 0190,$$

a result which is not significant, and from which we may conclude that the inclusion of reading matter in the pictorial test has not introduced any appreciable group factor in those tests.

(iv) *The effect of oral questions on pictorial tests.* Our other question, whether the giving of questions orally adds any group factor to those tests, may be similarly discussed. Taking p_1 and p_2 as the purely pictorial tests, and q_1 and q_2 as those which include the giving of questions orally, we find that

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[1] C. Spearman, *Nature of Intelligence*, pp. 64–65.

$$r_{p_1 p_2} r_{q_1 q_2} - r_{p_1 q_2} r_{q_1 p_2} = \cdot 0044 \quad \text{P.E.} = \cdot 0132.$$

Here again our result is not significant, and we may conclude that the giving of questions orally on the picture has not added any appreciable group factor to the test.

If this conclusion should prove to be right, it seems legitimate to say that instructions given orally for a pictorial test need not alter the test as a test of 'general intelligence.' But these instructions, of course, must be easy, and not involve the critical factor upon which the solution of the test depends. The giving orally of instructions seems to be the more practical method for a pictorial test, for it eliminates chance misunderstandings that may easily arise when instructions are conveyed by dumb show and gesture.

Having examined the possibility of the intrinsic group factors, *i.e.* those within the tests themselves, we now turn to those other influences which, though outside the tests, may yet invalidate the interpretation of our results unless their effect be noted. Such influences are those of sex, age, social status, race, etc. These, for convenience, we term extrinsic group factors.

(2) *Group factors extrinsic to the test.*

(i) *Sex.* In his work on the Binet tests[1], Dr Burt found that girls were better than boys by about three-tenths of a year, except at the age of ten. This advancement of the girls he finds due to their higher results in the linguistic tests, of which there are so many in the Binet-Simon scale.

The results of the present investigation show a similar difference when the marks for the oral tests gained by 99 boys are compared with those gained by 106 girls. The graphs below show the marks so obtained.

But the marks obtained by the same girls and boys, respectively, in the pictorial test (graphed below) point to the conclusion that boys do better at the pictorial test than girls.

We must now enquire how this result affects our main problem; can we say that we have here a group factor which will reveal itself by giving a positive bias to our main curve (p. 43)? We can say that it certainly would have done so, had our groups of subjects been composed of boys and girls together, instead of each group being either all girls or all boys. For then the correlations for the inter-oral tests would have been raised,

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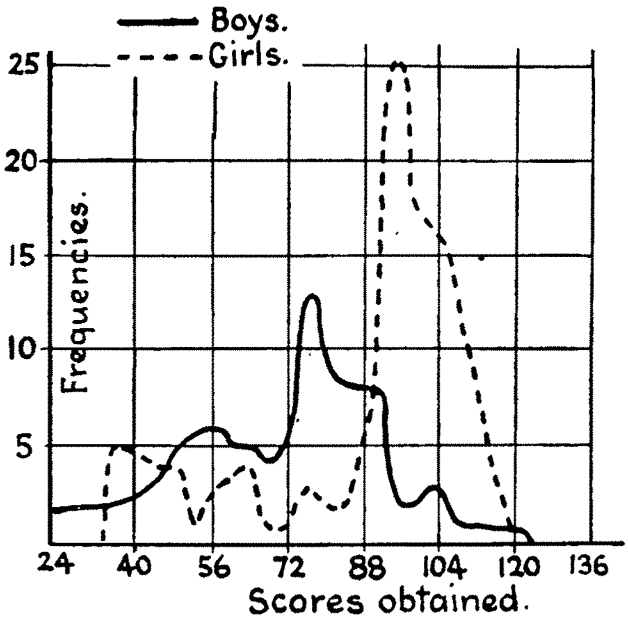
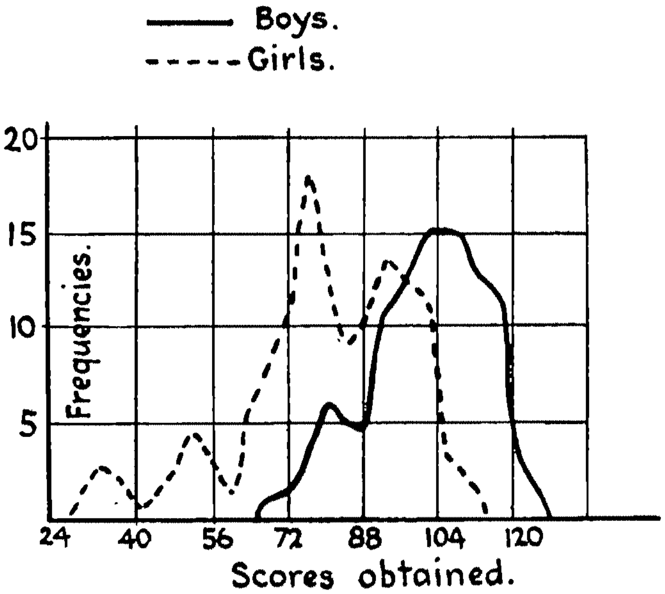
[1] C. Burt, *Mental and Scholastic Tests*, p. 193.

Oral test.



Pictorial test.



as the girls tend to do well and the boys badly in any two tests compared. Similarly, the inter-pictorial correlations would have been increased, for the girls tend to do badly and the boys well in any two tests compared, whereas the correlations for the inter-oral-pictorial tests would have been lowered, for both the boys and the girls tend to do well in the one and badly in the other of any two tests compared.

On the whole, then, we should have found that the results of

$$r_{o_1o_2}r_{p_1p_2} - r_{o_1p_2}r_{p_1o_2}$$

would have tended to be positive in a far greater degree than they are shown to be by our graph (p. 43).

(ii) *Age.* We now turn to consider the factor of age, and whether it accounts in any way for the positive bias of our main curve. In either of the tests, oral or pictorial, we should naturally expect to find that the older children do better than the younger. But, even if this is so, we have still to ask whether this superiority of the older over the younger children is equal for both types of tests. For, if our answer to this question is in the affirmative, age will not act as a group factor relative to one type of test, but as an absolute group factor throughout both the tests, and consequently it will not affect our results. But should we find that its influence gives greater superiority to the older children in one test than it does in the other test, we must conclude that 'age' is a relative group factor, when a comparison of the results of the two tests is being made.

We take, then, the scores of our youngest boys and girls, and graph them against the scores of an older group. In both cases the older groups do decidedly better in the oral test, but when we compared the marks of the pictorial test for these same children, we do not find this difference. Here the younger appear to do equally as well as the older children.

It would seem, then, from these results, that a group factor of age exists throughout the oral tests; when these tests are compared with a pictorial test, we find its influence present in the former and not in the latter. But even so, it is well to remember that the ages here compared are those of groups composed of different children, and in order to verify the conclusion that the influence of age is not present in a pictorial test to any appreciable extent, it would be necessary to test the same children year by year. It may be, that the amount of 'g' required to solve a pictorial test is small, and that the younger children have this in sufficient amount, while the growth of 'g' with age is coincident with the amount of 'g' required to solve an oral test. There appears to be, then, a development in linguistic ability as age increases from 9 to 14 years, but as far as we can judge the ability to solve a pictorial test does not, for our present subjects, increase in any appreciable degree with age. This being so, the influence of age is a possible group factor in our oral tests when they are compared with our pictorial tests.

But first we must ascertain how far we have eliminated this factor by using the average correlations of our groups of subjects. These groups were composed of school classes, and any attempt to divide them into two halves with respect to age always showed to a large extent that the older children were also the duller when judged by the results of the test as a whole.

Had the older children been also the more intelligent, we should have found the effect of age more marked; and there may be still some effect of this factor within our groups, for the difference in age in only three of them is less than twelve months. If we eliminate this effect by the use of partial correlations, we find that our observed correlations would be lessened for the inter-oral tests, while the inter-pictorial correlations would not appreciably alter, and the inter-oral-pictorial would be slightly raised. Thus the average values of $r_{o_1o_2}r_{p_1p_2} - r_{o_1p_2}r_{p_1o_2}$ would tend on the whole to be lessened. Therefore, by eliminating any influences of a group factor due to age, we should not tend to raise the median of our graph, but on the contrary, to bring it still closer to the zero value.

(iii) *Other group factors not affecting our main experiment.* Similarly other group factors may make invalid the interpretation of the results obtained by mental tests, unless they are excluded by the conditions of the test or eliminated statistically from the results. Such factors may be those of race, social status, besides others which may be incurred in the giving of the test, *e.g.* speed or practice. Of these, we have excluded most under the conditions of our tests, for example, those of speed and practice were the same for both tests, and, therefore, they will not affect the comparison we are making.

(c) *The result of eliminating 'group factors.'*

The elimination of the intrinsic group factor, in the first four of the oral tests, brought our median from ·021 to the insignificant amount of ·005, whilst the effect of eliminating the influence of age could only reduce this amount further still. Therefore, the formula

$$r_{o_1o_2}r_{p_1p_2} - r_{o_1p_2}r_{p_1o_2}$$

does, when group factors are eliminated, approximate to zero for the inter-correlations of the eight oral and six pictorial tests we have used. Thus the second condition required by our argument has been fulfilled, and we may legitimately conclude that mental tests using either verbal or pictorial material can be made to measure 'general intelligence.'

It is possible too, on these grounds, to account for the conclusion reached by some psychologists that verbal and non-verbal tests are testing something different. The method adopted by most of these has been to correlate the results of verbal and non-verbal tests[1], and to conclude, because of the low correlations obtained, that these tests must necessarily be testing different factors; or that the non-verbal test is the better test of general intelligence, for the verbal test measures, besides general intelligence, 'linguistic ability.' "From the evidence now in our possession we can now reasonably conclude that linguistic ability is an important factor in the score obtained by an individual in an intelligence test that is based largely on words and their uses[2]."

But these high inter-correlations between individual verbal tests may be due to the presence of group factors intrinsic to the test, for we have already pointed out the existence of one in our oral tests and traced its origin to the content of the test.

Both claims are right in certain respects; for as we have shown the verbal and non-verbal tests can be made measures of general intelligence, *i.e.* 'g'; but unless these tests are freed from the influence of group factors, their results will include in part the measurement of the specific ability to use that type of material. This we saw was the case when the group factor of age was allowed to remain, for with the increase of years came a greater development in the specific ability to use language, a development not seen in the use of pictures as the material of the test.

Though a pictorial test may then be substituted for a verbal one as a measure of 'general intelligence,' provided these group factors are allowed for, it must still be acknowledged that the development of linguistic ability is coincident with the development of intelligence, and, therefore, in the higher grades of intelligence the verbal test will be the more appropriate measure. But for younger children the practical importance of our result is obvious, for we may now substitute a pictorial test for a verbal one. Still, even here our problem is not ended, for the devising of such a test will necessitate the 'quest for conditions[3],' which will eliminate as far as possible any disturbing group factors, and make the final result of the test a measure of 'general intelligence' only.

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[1] Compare p. 29.

[2] S. S. Colvin, *Journ. Educ. Psych.* 1923, p. 9.

[3] James, *Principles*, p. 3.

5. SUMMARY OF CONCLUSIONS.

We may conclude that this investigation has established the fact that a verbal mental test measures the same general factor 'g' as does a test similar in form but non-verbal in material, *e.g.* pictures.

The assumption, then, that it was legitimate to substitute non-language pictorial tests in order to overcome an immediate difficulty due to a language disability has now been shown on more adequate grounds, to be correct. Nevertheless, we find no evidence to support the claim made by some psychologists that the non-verbal test is the better measure of 'general intelligence' because it excludes the language factor.

No such language factor we found to exist in any appreciable degree *throughout* our verbal tests when they were compared with our non-verbal tests; nor did we find the common quality of pictoriality forming a 'group factor' in the non-verbal tests.

The high values obtained in correlating verbal tests among themselves may be almost entirely due to the common general factor 'g'; but, on the other hand, they may be only in part due to 'g' and in part to the presence of a 'group factor' or 'group factors.' Unless such 'group factors' are detected, the interpretation given to the results may be invalid. The elimination of 'group factors' becomes essential when verbal and non-verbal tests are used for the purpose of comparing their relative capacity as measures of 'general intelligence.'

The 'group factors' we termed for convenience 'intrinsic,' *i.e.* due to the test itself, or 'extrinsic,' due to the influences outside the test. One of these 'intrinsic group factors' revealed itself in a marked degree in four of our verbal tests: its detection and elimination showed the fallacy of concluding that the source of a high correlation between verbal tests is necessarily their verbality.

Of the 'extrinsic group factors,' sex and age play an important part when a comparison of two such types of tests is being made.

The introduction of easy reading matter into the pictorial completion test did not bring any significant group factor into existence; nor did the fact that we gave oral questions on a picture. We, therefore, conclude that questions or instructions given verbally might be substituted in non-verbal tests for dumb show and gesture, provided these questions or instructions are easy and do not involve the critical factor upon which the solution of the test depends.

Considering now the pictorial tests alone, the results of our experiments indicate that the following are the tests which serve to distinguish most satisfactorily the bright from the dull subjects (accepting, that is, the teacher's estimate of their 'intelligence' as our standard): Completion, Classification, Sequence, Questions, and Analogies Tests.

From an examination of the errors made in these pictorial tests by our subjects, we conclude:

(1) that the chief source of error throughout appears to be failure to educe a relation or find a correlate, and to select, instead of the correct picture, one that is associated closely with the picture most interesting to the subject;

(2) that the relation of similarity between *characters* of the pictures as fundaments is more difficult of eduction than is the relation of similarity when the pictures themselves act as the fundaments;

(3) that the relation of similarity is educed at an earlier age than is the causal relation.

With regard to certain other conclusions indicated by this investigation, we consider further experiment necessary before these can be regarded as established:

(1) Do spatial tests depend to any extent for their solution on some specific factor, and if so, is this factor more predominant than the general factor 'g'?

(2) Does the ability to solve a pictorial test develop with age? The conclusion we reached that the younger did as well as the older children was based on different groups of subjects of different ages, but definitely to establish the above, the same children must be tested throughout several years.

Or is it that the amount of 'g' required to solve a pictorial test is small and younger children have not this sufficient amount? If so, at what age is this amount attained?

(3) When the source of a group factor is the 'content' of a verbal test, is this due to (*a*) a similarity in the fundaments used, or (*b*) a similarity in the relation educed?

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A NOTE ON THE EFFECT OF POLARISATION IN PSYCHO-GALVANIC EXPERIMENTS

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In an article by R. H. Thouless on the Psycho-Galvanic Phenomenon in this *Journal*, July 1925, there is a footnote on the effect of polarisation on the apparent resistance of the subject. A method of experiment is suggested which is stated to prove that the effect of polarisation is small (being of the order of about ·5 per cent.). Further experiments have shown that this conclusion is wrong. The general results of the above paper are in no way invalidated by this error in the footnote, but it seems worth while to communicate a short note on this point in order that other workers may not be misled.

When we afterwards attempted to measure the polarisation current by means of a string galvanometer with a glass string, examination of the photographs showed that, even in the short period of 0·005 sec. required by the glass string to take up its position, the polarisation must have altered by an amount so great that no reliance could be placed on results obtained even by a galvanometer with as short a period as this.

The rapidity with which the polarisation changes makes hopeless the attempt to measure the true resistance of the subject and his true polarisation by any of the three obvious methods. These are:

(*a*) The 'secohmeter' method in which, in rapid alternation, an external current is passed through the subject and then the external current is cut out and the polarisation current from the subject is passed through a string galvanometer.

(*b*) Salomonson's method (also using a string galvanometer) in which an external resistance is inserted into the circuit and short-circuited in rapid succession, and the subject's resistance is calculated from the formula $S = \frac{Ri_2}{i_1 - i_2}$.

(*c*) The ordinary method in which the resistance is measured by means of a slow-moving galvanometer and an external current rapidly alternated by means of a mechanically driven commutator.

Method (*a*) fails because the polarisation rapidly decreases as soon as the external current is removed, and examination of photographs obtained with a sensitive string galvanometer shows that, even during the very short interval of time necessary for the string to come into position (0·005 sec.), the change must have been so large that no reliance can be placed on the initial reading of the polarisation (or even on a value obtained by a backward prolonging of the observed curve of decline of the polarisation current).

Method (*b*) appears to be a more hopeful method, for there is only alteration in the amount of the external current and not in its direction, so there is relatively small change in the polarisation current. This advantage is, however, counterbalanced by the fact that a very small change in the value of the polarisation current during the time taken by the string to take up its new position will make a very large change in the calculated value of the subject's resistance. Examination of photographs obtained by this method seems to show that its error is no less than that of method (*a*).

The objections to method (*c*) can also be shown by examination of photographs of the movement of a sensitive string galvanometer in circuit with an alternating current through a subject's hand. These show that, instead of polarisation being eliminated by an alternating current, there is a large E.M.F. of polarisation reversed with each reversal of the external current. This reversal does not take place along a straight line but along a line falling much more rapidly at first. The result is that the effect of polarisation, even with an alternating current, is to make the resistance of the subject appear greater than it actually is. This effect is probably heightened by the dissipation of polarisation during the short gap at the moment of reversal.

The effect is, of course, decreased by increasing the rapidity of the alternations, but this has not yet given a satisfactory method of measuring the effect. With the circuits and speeds (not greater than 50 revs. a second) which we have used so far, the effect of polarisation is considerable. If a very large speed of alternation were obtained by other methods (*e.g.* by the use of thermionic valves), it is highly improbable that the other conditions of the passage of the current would remain the same. A rapidly alternating current confines itself largely to the surface of the conductor, and results obtained with such a current would give no information about the resistance of the subject to a direct or a low-frequency current. Even if the P.G.R. did not exist at all for such rapidly alternating currents (as has been asserted by Prideaux) we could draw no conclusion as to the cause of the P.G.R. observed when using an ordinary current.

The large effect of polarisation on the apparent resistance of a subject when this is measured by an alternating current of low frequency is indicated by the following figures, which show the apparent resistances of three subjects when measured at different speeds of the commutator. It will be noticed that a difference of 30 per cent. may result from taking measurements at 6 and at 36 reversals per second. We have also found that the percentage difference in apparent resistance is greatest when the subject's resistance is highest.

The subject, an external E.M.F. of 2 volts, and a moving coil galvanometer were in series. The current was alternated through the subject but not through the galvanometer by means of a mechanically driven commutator. The apparent resistance between the electrodes on the palm and the back of the hand was measured with the commutator at a speed of 6 revs. a second, then at a higher speed, and again at the original speed.

In the following tables, Speed 1 is a speed of 6 revs. a second, Speed 2 one of 36 revs. a second, and Speed 3 one of 23 revs. a second. It will be seen that in all cases an increase of the speed of the commutator causes a decrease of the apparent resistance. The magnitudes of the resistances are given in ohms, and also the decrease of resistance, expressed as a percentage of the mean of the apparent resistances at Speed 1 immediately before and after.

TABLE I. *Effect on Apparent Resistance of a Change in Speed of Commutator from* 6 *revs./sec.* (*Speed* 1) *to* 36 *revs./sec.* (*Speed* 2).

	Resistance at Speed 1. Before	Resistance at Speed 2	Resistance at Speed 1. After	% Decrease of resistance
Subject A	24,870	17,560	24,710	29·2
	24,560	17,480	24,400	28·6
	24,400	17,320	23,950	28·4
	23,950	17,170	23,660	27·9
	23,660	16,870	23,370	28·3
Means for A	24,290	17,280	24,020	28·5
Subject B	24,100	16,720	23,950	30·4
	23,950	16,440	23,660	31·4
	23,370	16,300	22,820	29·6
	22,820	16,030	22,300	28·9
	22,170	15,770	21,550	27·9
Means for B	23,282	16,252	22,856	29·6
Subject C	9,432	7,993	9,523	15·7
	9,614	8,091	9,707	16·2
	9,804	8,123	9,804	17·2
	9,902	8,191	9,851	17·1
	9,902	8,224	9,851	16·7
Means for C	9,731	8,124	9,747	16·6

TABLE II. *Effect on Apparent Resistance of a Change in Speed of Commutator from* 6 *revs./sec.* (*Speed* 1) *to* 23 *revs./sec.* (*Speed* 3).

	Resistance at Speed 1. Before	Resistance at Speed 3	Resistance at Speed 1. After	% Decrease of resistance
Subject A	23,100	18,650	23,100	19·3
	23,100	18,480	22,820	19·5
	22,820	18,220	22,820	20·2
	22,820	18,130	22,560	20·1
	22,050	17,480	22,050	20 7
Means for A	22,778	18,192	22,670	20·0
Subject B	21,550	17,480	21,320	18·5
	21,200	17,400	21,090	17·8
	21,090	17,170	20,860	18·2
	20,860	17,020	20,640	18·0
	20,530	16,870	20,420	17·6
Means for B	21,046	17,188	20,866	18·0
Subject C	9,877	8,730	9,877	11·5
	9,877	8,730	9,877	11·5
	9,902	8,768	9,902	11·5
	9,902	8,806	9,951	11·1
	10,000	8,845	10,050	11·8
Means for C	9,912	8,776	9,921	11·5

Although the above methods have proved unsuccessful, we are hopeful that, by other methods, it will be possible to make accurate measurements of the subject's resistance and of his polarisation.

When the resistance of a subject is measured by means of a direct current from an external source of voltage the value of e/i which is directly read may be called his 'apparent resistance.' Actually it is an amount much greater than his true resistance.

If the true resistance of a subject $= R$,
his apparent resistance $= R'$,
the imposed E.M.F. $= e$,
and the E.M.F. of polarisation $= p$,
then

$$R' = R\frac{e}{e-p}.$$ [1]

In psychological experiments on the P.G.R., it is often desired to express the actual value of the change in the subject's resistance either in ohms or as a percentage of his total resistance. For this purpose, the psychologist is justified in measuring the value R' and its changes without considering the physical complexity of their origin. It is necessary to remember, however, that the value R' so obtained is not only a function of the true value of R but also of e and probably also of the size of the electrodes, the concentration of the electrolyte used, and the position on the body in which the electrodes are placed. It is possible, although not certain, that the polarisation is completely determined by these factors.

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[1] In this equation we are neglecting the small 'somatic current' which flows from the back of the hand to the palm. This is found to be negligible except for very small values of e.

This means that in order that any numerical measurements of the percentage amount of change in the P.G.R. may be comparable with those obtained by other workers, it is necessary that all these factors should be recorded. The value of R', for example, will not be the same when the subject's resistance is measured on the Wheatstone bridge and when it is measured in a direct circuit. Also, when measured on the Wheatstone bridge circuit, R' will vary with various values of the other resistances in the circuit.

R' is, in short, a value which depends not only on the actual value of R but also on all the other conditions of the circuit. Provided, however, that all of these are noted and are kept constant, it seems quite justifiable to use the changes in R' as a numerical measure of the P.G.R., until a reasonably simple method of measuring R itself can be evolved.

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PSYCHO-GALVANIC STUDIES ON SIXTY-FOUR MEDICAL STUDENTS

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IN the studies here reported our aim was to determine in a large number of normal individuals some of the typical features presented in psycho-galvanic curves and to relate them to the characteristics of the emotional behaviour observed through other means (daily reactions and activities, objective and subjective description). We thus were interested in the psycho-galvanic experiment as a possible indicator of the emotional make-up, and we were only little concerned with topics reacted to or with an analysis of the phenomena which make up an emotion. In spite of the uncertainty concerning the mechanism involved, there is general agreement that this reflex can be used for reliable registration of affect reactions. It is well, however, to be aware of the limits of both the test and its application. The term 'emotion' designates a complex function of the entire personality; the psycho-galvanic reflex is only a part-function and we know comparatively little about the inter-relations.

The component of an emotion which can be observed by means of the galvanic phenomenon does not give us, of course, any indication as to the specific type of the underlying affective state. It does not tell us whether we have to deal with embarrassment, surprise or amusement. The specificity of an emotion is mainly expressed in movements and postures, in complex functions of the skeletal musculature denoting a specific motor orientation in space and giving evidence of definite action tendencies and attitudes of the whole organism toward the environment. This important and specific—though often repressible—involvement of the voluntary musculature is not registered in the electrical reactions at all. The fact, therefore, that only one single component of the complex 'emotion' is expressed in the galvanic waves marks restrictions in the application and interpretation of the psycho-galvanic reflex; it does not invalidate the positive findings.

This experiment has the advantage of satisfying to a rather high degree the demand for objectivity. The reactions registered are beyond the control and arbitrary influence of the person under test and the recording cannot be affected by the bias of the observer.

A study of such functions of the entire organism as the emotions represent requires an experimental arrangement which suggests an actual and not too unusual life-situation. We attempted to provide such circumstances, but we are aware that an approach to personality reactions through a procedure employing mechanical devices must remain quite artificial.

Our experiments represent the responses of various individuals to a constant set of stimuli to which there could have been no habituation. The time of experimentation was comparatively long (thirty to forty minutes), and thus we expected that the test might give a few glimpses of the emotional flow in the subjects under observation.

Though our work includes tests of normal subjects and of neuropsychiatric patients, in this paper we shall report only the experiments on a group of normal persons. We chose as environmental influences sensory stimuli (tactile and auditory), threats and a list of words suggestive of emotionally significant life-situations. We did not follow any one of the frequently used association lists as we wanted to include terms taken from a greater variety of human experience. These words characterised various aspects of such situations as: family, sex, social standing, success, money, religion, moral standards, self-esteem. The stimulus words were placed in the list so as to effect a sort of balance: words indicating similar situations were set apart and care was taken not to place together terms which would through their mere combination arouse an emotional response. The same standard list was not used for all subjects; for the patients, words were inserted alluding to the circumstance of their sickness, to possible fears and apprehensions; for the students here studied words were included suggestive of their medical and social interests. Lists thus prepared might seem to contain too many words of emotional value, but experience showed that as a rule only a small number of the words evoked responses in any one subject. A high frequency, therefore, does not indicate an inappropriate choice of the stimuli, but is due rather to special features in the emotional constellation of the individual.

In the following pages I shall report the findings on sixty-four first-year medical students who were asked by Dr A. Meyer to undergo this experiment as a part of their course in psycho-biology. Participation was not compulsory. The data here reported may be useful as a basis of comparison for other studies on both normal people and patients.

Method of Experimentation.

The technique followed in the experiments was that developed by C. P. Richter in the Psychological Laboratory of the Henry Phipps Psychiatric Clinic; it deviates somewhat from the methods employed by other workers (Veraguth[1], Gregor and Loewe[2], Wechsler[3], and others).

It seemed advisable to keep the conditions of the experiment as simple as possible and to have the subject in a passive and receptive state. Accordingly at the beginning of the test each subject was asked to avoid unnecessary movements and to listen without verbal response. Our feeling was that the request for a reaction word would introduce an additional factor and thus complicate the experiment and its interpretation. Outside noises were eliminated to a large extent by the double doors of the experiment room, and other stimuli not belonging to the experiments were excluded so far as the circumstances allowed. During the experiment the room which was in semi-darkness was divided into two parts by a curtain. On one side was the individual under test, seated in a comfortable arm-chair, on the other were the galvanometer and the camera. The experimenters also remained behind the curtain out of the subject's sight (except when sensory stimuli were given).

The subject's arms were flexed at the elbows, the forearms were supported by and fastened in arm-rests which prevented movements. The fingers were extended, the palm being held in a vertical position. After thorough cleaning of the hands with alcohol and ether, the electrodes—screwed on the arm-rests—were applied to the palms and backs. These electrodes which were made of zinc, 1½ inches in diameter, were connected with the skin by a paste of kaolin and saturated solution of zinc sulphate and thus rendered non-polarisable. No external current was introduced. In the circuit was a switchboard so that the galvanometer could be connected with the palms or with the backs of both hands or with the back and palm of either hand. The instrument used was a modified (Hindle) Einthoven string galvanometer, the whole arrangement, with arc light and photographic recording, being the same as that employed for electro-cardiography.

Before each experiment the string of the galvanometer was so standardised (without subject in circuit) that the introduction into the circuit of 1 millivolt corresponded to 10 mm. deflection of the shadow of the galvanometer string on the scale of the camera. The individual

[1] C. Veraguth, *Das Psychogalvanische Reflex-phaenomen*, 1909.

[2] A. Gregor and S. Loewe, *Zeitschr f. d. ges. Neurol. und Psychiatrie*, 1912, XII, 411.

[3] D. Wechsler, *The Measurement of Emotional Reactions*, 1925.

under test was then connected with the circuit by two of the four terminals, employing successively those of the same hand (right and left) and then those of corresponding areas on both hands (backs and palms). For each of the four connections the resting current and resistance were first measured; the former by determining the amount of voltage necessary to compensate for the deflection caused by the patient at rest, and the latter by recording the number of millivolts necessary to deflect the string 10 mm. From this (v) and from the known resistance (x) of the galvanometer string, the resistance of the subject can be derived by means of the formula $r = x\,(v - 1{\cdot}0)$.

After determining the resting current and the resistance the back and palm of the hand that shows the lower resistance was connected in the circuit and the string was so re-standardised (with subject in circuit) that the introduction of 10 millivolts caused a deflection of 25 mm. This same standard of sensitivity has been used for all of our psycho-galvanic work with various groups of individuals.

In addition to the psycho-galvanic curve a pneumographic record was taken. An air cushion attached to a belt was applied to the chest of the individual, the air was conveyed by a rubber tube to a tambour with a lever whose movements were registered on the rolling film of the camera together with varying positions of the string of the galvanometer. On the same film paper the time in seconds was recorded by a Jacquet clock, while a signal marker connected with an electrical circuit was used to indicate exactly the time when the stimuli were presented.

The words were read by one of the experimenters, without special emphasis or intonation, at intervals of 8 to 12 seconds. The same experimenter regulated also the arc light, whereas his assistant recorded the stimuli given and attended to the string and to the camera. If an especially marked reaction occurred the interval before the following word was extended so as to allow the string to return to its base line. If the waves continued over a longer period a few 'neutral' words were inserted (for instance: window, glass, green, house, etc.).

After reading the list of fifty-five words a quiet period of about 2 minutes was inserted. An automobile horn then was sounded and a second reading of the list of words followed. After this, a few tactile stimuli were given: a pin-prick on the right forearm, a pin-prick on the neck; the threat of a pin-prick on the left forearm and finally the carrying out of this threat. Then the automobile horn was sounded a second time. When the string had returned to its original position, the subject was asked to take a few deep breaths. This was followed by the request that the student imagine a situation with emotional involvement. He was asked to say 'now' as soon as he was aware of an emotional swing connected with his play of fancies. At the end of the test the string was re-standardised (1 mv. = 10 mm. without the subject in circuit) for a second reading of the resting current and resistance.

After the experiment the subjects always were shown the stimulus list and were asked to mark those words which they remembered to have elicited affective responses or definite associations and also those to which they would have expected a reaction.

Method of Evaluation.

For the purpose of describing the photographic records of the experiments in terms of formal characteristics, we singled out such features as: the various types of waves (determined by their time relation to the stimuli), their relative frequency, the amplitude, the shape of the galvanic responses (degree of consistency in form, as present throughout the entire curve) and the regularity of the respiratory movements. The underlying idea in this procedure was that some of these formal characteristics or their combination might be found empirically to occur in individuals with specific emotional traits; these features of the curves could then be used as indicators of specific traits in the temporary affective behaviour, or even in the more permanent emotional constellation of individuals under test. The topics reacted to, therefore, were of only secondary interest.

The following indicators were used:

The *frequency and timing* of the reactions; all waves with an amplitude of 1 mm. or more were counted as reactions. These were differentiated, on the basis of their relation to the stimuli, into the following types: direct, late, disconnected, secondary and spontaneous. For the classification we considered the setting of the entire curve as well as the characteristics of the individual waves. The responses to stimuli other than words were evaluated separately.

We class as *direct* reactions (Fig. 1) those which follow the stimulus within from 1 to 4 seconds.

If the reaction begins later than 4 seconds after the stimulus we call it *late* (Fig. 2). The distinction between direct and late reactions was based on the fact that the great majority of the responses occur between 1½ and 3½ seconds after the stimulus and because it appeared advisable to class the more retarded reactions separately. We did not include, however, among these delayed responses any that occurred later than 8 seconds after the stimulus.

Waves occurring after an interval of 8 seconds are classed as *disconnected* (Fig. 3). They can hardly be taken as direct responses to the stimuli and we consider them as arising in the main endogenously. Any connection which might exist would be so indirect that the designation 'disconnected' seems fully justified for our purposes.

The small fluctuations which often appear after direct and after late reactions are called *secondary* waves (Fig. 4). They are not separate reactions but present only the overflow of a particularly strong response. The galvanic skin reflex stirred up by the stimulus apparently does not quiet down immediately but causes a few after-waves. In the estimation of this type of fluctuation we counted as one secondary wave all the fluctuations which occurred between two consecutive stimuli.

In some of the records there are waves which cannot be classified in the groups already mentioned. These waves are not directly connected with a stimulus, they usually are not very marked and are fairly uniform in amplitude. We call them *spontaneous* waves (Fig. 5). For their evaluation it is important to consider the 'setting' of the curve; the 'quiet period' especially gives valuable information here. If many spontaneous waves occur in this free interval we expect similar fluctuations also during the time of stimulation and they might then be wrongly referred to word stimuli. A recognition of this prevents the confusion of spontaneous waves with direct reactions. Spontaneous waves are distinguished from disconnected reactions by the smaller amplitude of the former and by their occurrence in series. They were counted on the same basis as secondary waves.

We took note also of the *amplitude* of the galvanic reactions, both the maximum and minimum, and we determined the amplitude specifically for the responses to horn, threat and prick.

Another formal indicator is the *shape* of the reactions; that is, the degree of consistency in the form of the waves throughout the test. Five degrees of regularity in shape were distinguished based on inconsistancies in the form of the waves and disregarding variations in amplitude. Fig. 6 shows a curve with waves of irregular shape which may be compared with Fig. 7 presenting waves of regular shape but varying in amplitude.

In addition we tried to single out characteristic features of the *respiration*, taking into account the frequency, amplitude and form throughout the entire test. The respiratory curves also were grouped according to the five degrees of regularity given on Fig. 14. Figs. 1, 2, 6 give regular respiratory curves; Figs. 3 and 8 irregular curves.

The stimulus words were not written on the records and they could therefore in no way influence the classification of the reactions. In order to control this classification it was made independently by the writer and by an assistant. It was found that the differences in these two independent evaluations (based on the principles outlined in the preceding paragraphs) were less than 1 per cent. The mistakes caused by a personal interpretation thus were practically negligible.

Results.

The records of the two sexes were treated together as the number of female subjects was not large enough to warrant separate treatment, and as no marked differences between the curves of male and female individuals were observable.

Before considering the features which especially interested us it may be mentioned that resting current and resistance findings could not be linked up with emotional traits of the individuals tested. Neither were there definite correlations between the behaviour of the resistance (consistent amount or consistent changes) and the features of the galvanic curves. A slight correspondence was noted only in the fact that low resistance tended to be combined with a number of reactions exceeding the average, though a high resistance does not exclude the possibility of numerous responses.

Other data secured through these experiments were found to be of little value for a study of the emotional make-up; they, therefore, will not be treated here. We shall discuss in another communication the reactions caused by the request to imagine an affect situation and the relation of this feature to other phenomena observed in the individuals experimented on.

Though all types of galvanic responses were present in this group there is a marked preponderance of direct reactions over the other types, as may be seen in Figs. 9–12. Fig. 9 gives the frequency of the various types as percentage of the stimuli given, while Fig. 10 shows the number of individuals (per cent.) in whom each type occurred. The maximum number of direct responses was 71, the minimum 6, only one individual gave no response at all to the verbal stimuli. The subjects were quite evenly distributed between the two extremes, the average frequency for the entire group being 34. Fig. 12 shows the direct responses as percentages of the total number of reactions for each subject. The

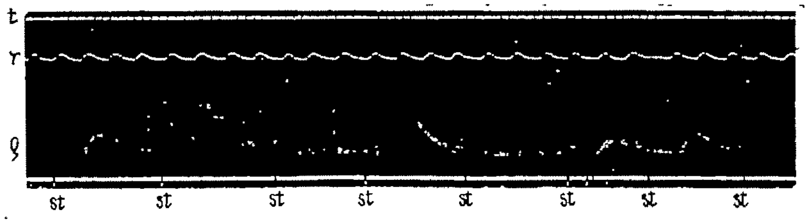

Fig. 1. Part of a film recording: the time of stimulation (*st*), the galvanic curve (*g*), the respiratory curve (*r*) and the time in seconds (*t*). The galvanic reactions follow the stimuli directly, within an interval of 1½–2 seconds. They vary somewhat in amplitude but are of regular shape. The respiratory curve is regular.

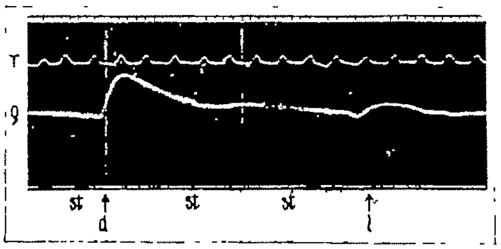

Fig. 2. Record showing a direct reaction (*d*) and a late reaction (*l*) occurring later than 4 seconds after the stimulus word (*st*).

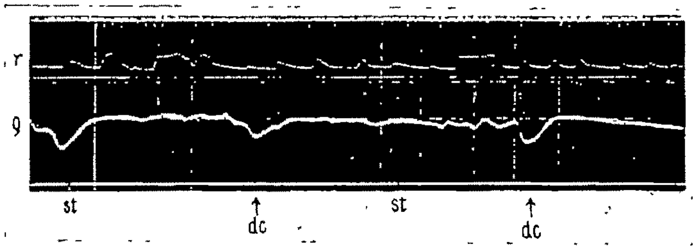

Fig. 3. Record with two disconnected reactions (*dc*). The marked deflections occur later than 8 seconds after the respective stimuli (*st*). There is no plausible connection with the preceding stimulus word. The shape of the galvanic reactions tends toward irregularity and the respiratory curve is markedly irregular. (This record was taken from a patient.)

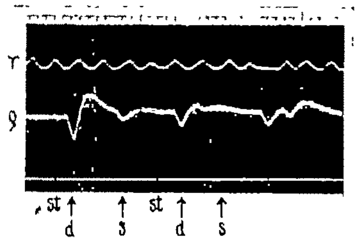

Fig. 4. Record showing secondary waves. The galvanic reactions (*d*) which follow the stimuli (*st*) directly are each succeeded by a secondary wave (*s*).

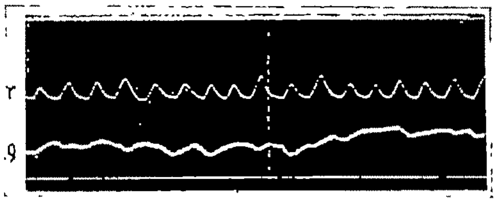

Fig. 5. Record with spontaneous waves; these are numerous and of moderate amplitude. No stimuli were given.

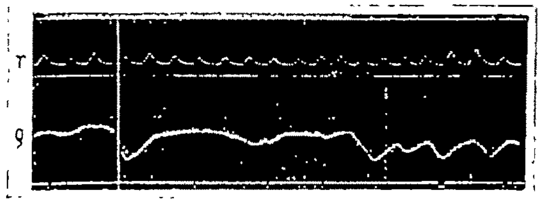

Fig. 6. Record showing galvanic reactions of irregular shape, a feature which becomes more striking if the entire record is taken into account. The connection with the stimuli is uncertain. Respiration regular. (This record was taken from a patient.)

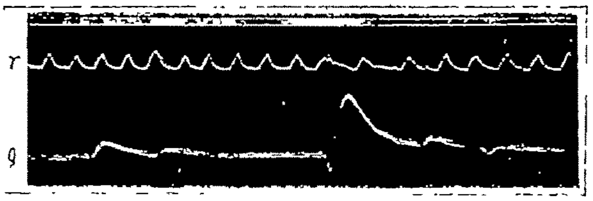

Fig. 7. Direct reactions differing in amplitude but of regular shape. The strong reaction is accompanied by a slight irregularity of the respiratory curve.

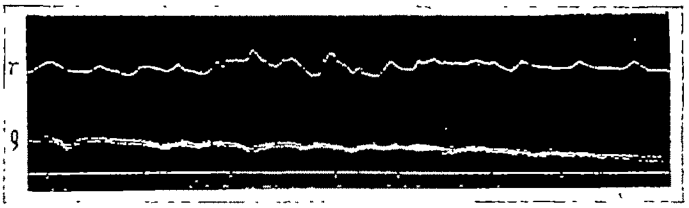

Fig. 8. Shows marked irregularity of the respiratory curve. The galvanic reactions are direct, of small amplitude, some of them followed by secondary waves.

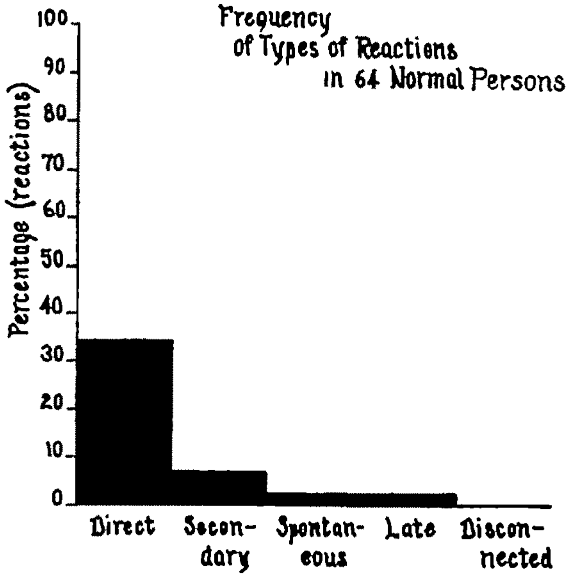

Fig. 9. The vertical axis denotes the frequency (as per cent. of stimuli given) of the various reactions indicated on the horizontal axis (averaged for the entire group).

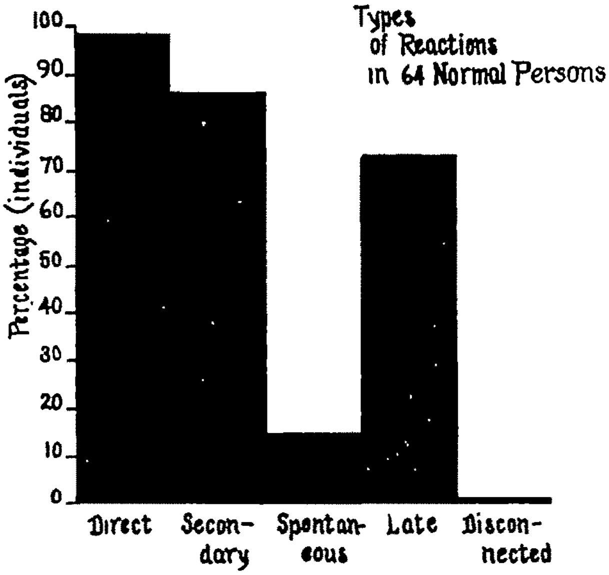

Fig. 10. On the vertical axis is marked the percentage of the total number of individuals who give the various types of reactions indicated on the horizontal axis.

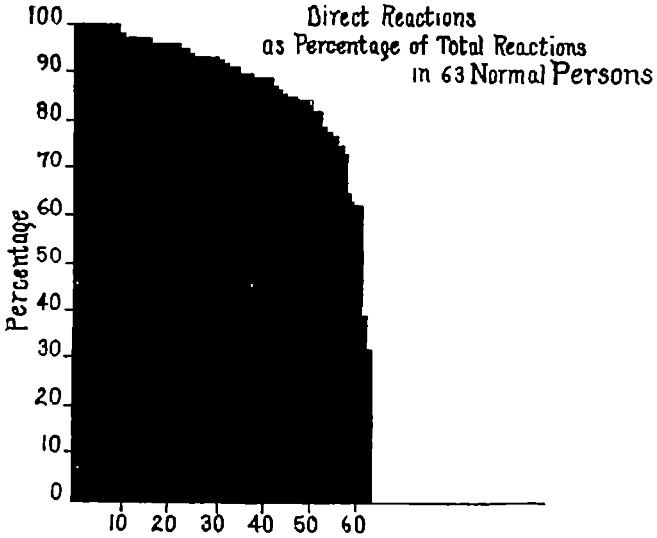

Fig. 11. On the horizontal axis the individuals are grouped in deciles according to their number of direct reactions. On the vertical axis the averages of late, secondary and direct reactions are recorded for each group.

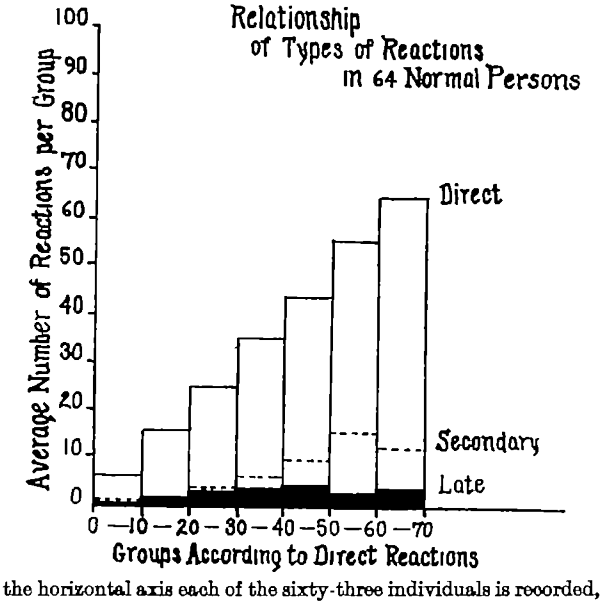

Fig. 12. On the horizontal axis each of the sixty-three individuals is recorded, the sequence corresponding to the proportion (as per cent.) of direct reactions to the total number of the responses. The one individual not included gave no reaction at all.

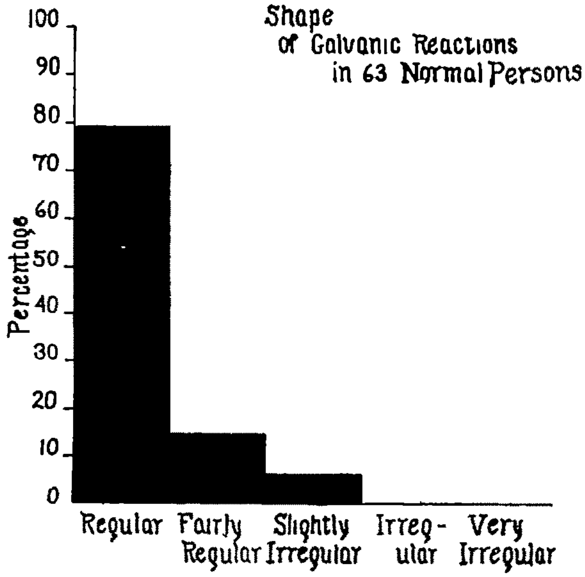

Fig. 13. The vertical axis represents the number of individuals (as per cent. of the total number of subjects) who gave the various degrees of regularity in the shape of their galvanic curve (as indicated on the horizontal axis).

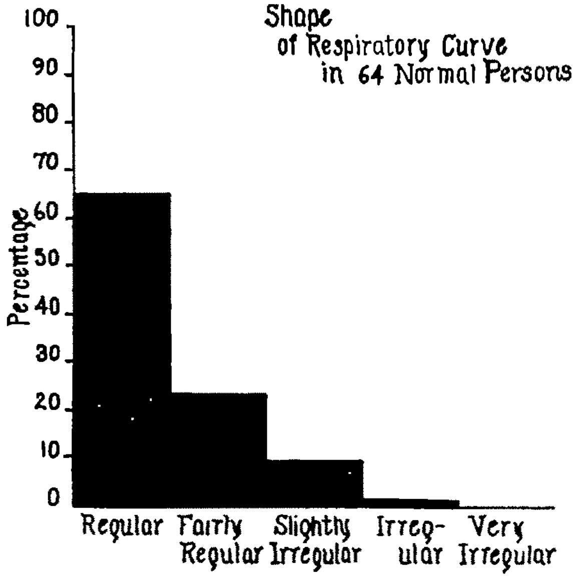

Fig. 14. The vertical axis represents the number of individuals (as per cent. of the total number of subjects) who gave the different degrees of regularity in the shape of their respiratory curves (as indicated on the horizontal axis).

preponderance of direct reactions is very obvious; they amounted to over 80 per cent. of the total responses in all but ten of the individuals, and in only two cases were they less numerous than other types (spontaneous).

A few secondary waves were found in a large number of individuals. The spontaneous and late reactions occurred with an equally low average frequency. A few late reactions were present in most of the records; spontaneous waves, on the other hand, were found in relatively few individuals, but there in great number (as illustrated in Fig. 10). The frequency of late reactions was not proportional to the number of direct responses, in the majority of the records it was between 2 and 3 per cent., as may be seen in Fig. 11 (where the individuals are grouped according to the frequency of direct reactions). Disconnected reactions occurred in only one subject.

The shape of both the galvanic and respiratory curves was predominantly regular (Figs. 13 and 14); a slight tendency toward irregularity was found in only a few cases (more marked in the respiratory curves). These slight irregularities of the respiration and of the galvanic waves were not both found to occur together in the same individuals.

Correlation with Emotional Make-up.

If we consider the students tested as a collection of normal individuals —and they are probably as 'normal' as the individuals of any similar group—we can consider their psycho-galvanic behaviour as indicative of emotional features present under such experimental conditions in the rank and file of normal persons. The average and range of the experimental findings in this group may be taken as a basis of comparison for single individuals as well as for other groups. The characteristics for the different individuals tested vary, but not beyond definite limits; the passing of these must attract our attention and is suggestive of underlying emotional peculiarities. Since a number of factors enter into our experiment which cannot be exactly estimated, we cannot expect a very close and consistent correspondence between emotional constellation and galvanic findings. Therefore, less significance is placed on the absence of features in the curves and more on the characteristics which are present. As repeated examinations of the same individual at different times have shown little variation in the type of the curves, we conclude that the momentary constellation does not hinder the appearance of features which are typical for the individual. This makes it possible to draw conclusions concerning emotional make-up from even a single test.

For the characterisation of the emotional make-up it is valuable to know: (1) how easily emotional responses are evoked; (2) how strong and enduring they are; (3) how closely they are connected with the environment; (4) whether the emotions are well harmonised and consistent among themselves; (5) whether they are 'adequate' and of what type.

We can obtain information from the psycho-galvanic reflex on several of these points. The frequency of the reactions gives a measure for emotional responsiveness; the amplitude indicates the intensity of the underlying affective state, a feature which is also expressed in the tendency to prolonged responses (secondary waves). Spontaneous waves are indicative of emotional conditions that are not dependent upon stimuli presented; they suggest that the individual is easily stirred up, that his affects are apt to endure and are not readily modified by outside stimuli. The endogenous nature of affects and their independence of actual environmental stimuli are even more apparent in the so-called 'disconnected' reactions, *i.e.* marked swings of the string without any plausible relation to the stimuli. The frequency of the responses is not only a measure of the general emotional sensitivity; it may in some cases be expressive also of what might be called the emotional balance. Individuals of harmonious emotional organisation give, in the same setting, fewer responses than those of more conflicting and unstable emotional make-up. Irregularity in the shape of the galvanic waves is another sign which points to an unbalanced and unstable emotional behaviour, as concluded especially from experiments with pathological subjects not discussed in this paper.

Whether any information can be gathered from the formal elements of the curves alone as to the type of emotions (embarrassment, anger, amusement, etc.) is extremely questionable, and it seems to be quite impossible to obtain any hint as to the 'adequacy' of the response.

The frequency of the reactions showed more variation than any other feature in the curves, indicating—as would be expected—a marked variation in affective responsiveness of the various individuals tested. All except one subject gave responses to the verbal stimuli. There were, however, large individual differences in the total number of responses. The average for the group was 34 per cent. with the variation from 6 to 71. It is especially interesting to study by another method the emotional constellation of those individuals who stood at the extremes. I was allowed to use the data furnished by the students' autobiographic descriptions of their emotional and general personality make-up, furnished in the personality studies written as a part of Dr A. Meyer's course in psycho-biology[1].

The smallest numbers of reactions (0, 6, 8) were given by individuals characterised as follows:

1. *No response.* Generally cheerful.—Likes everybody; says that he never disagrees with anybody; socially well-adapted; has no special problems.—Describes his temperament as happy, foolish, joking, kid-like.—Emotions of others stir him but little.
2. 6 *reactions.* Enthusiastic.—Accepts things always as they are; not hard to please.—Time filled with actual tasks; constant in his work and much interested in everything he does.—Says he has no imagination. —Conventional; no difficulties of adaptation.
3. 8 *reactions.* Interested in things.—Clock-like regularity in daily activities.—Says he reacts to things with calmness and composure.—Ambitions and activities well-adapted in a self-confident and conventional way.

The above data show that these three individuals are not of the type which is easily stirred up emotionally; they are well-balanced and give evidence of a low emotional responsiveness.

Four individuals gave a very large number of responses (71, 70, 67, 66):

1. 71 *reactions.* Timid and over-modest.—Many interests; more complex character than the average.—Describes temperament as outgoing, active, happy, but over-sensitive.—Stirred deeply by moods of others.
2. 70 *reactions.* Of meditating nature, musical.—Difficulty in mixing; shy with girls.—Easily embarrassed and influenced by slight disturbances. —Over-conscientious.—Says he is very emotional, moody, self-centred, acting on the impulse of the moment.
3. 67 *reactions.* Cheerful.—Inclination to hot temper but controlled. —Uneasy with strangers.—Easily homesick and 'blue,' does not always get over discouragement quickly.
4. 66 *reactions.* Generally in good spirits but bashful; complains of feelings of inferiority; at times retiring.—Activity in spurts.—Says he

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[1] In the personality studies the students gave a description of their own temperament and their habitual affect reactions. Their emotional tendencies were apparent also in the accounts of their activities, interests, habits, contacts and problems of adaptation. These personality data were treated as strictly confidential and the writer had access only to those which were not marked 'personal.'

is influenced to a great extent by the esteem of others, that he is diffident and self-depreciating.—Tendency to sarcasm.

The characteristics given above show that the four individuals with the maximal number of galvanic responses are of an emotionally sensitive type, easily stirred up and with some evidence of emotional instability. The difficulties of social adaptation found to a certain extent in these four subjects is closely related to their emotional unrest and their heightened affective responsiveness.

The correlation of these seven emotional pictures with the number of galvanic reactions shows that a low frequency is found with a low affective sensitivity and an evenly balanced emotional constellation, whereas a high frequency denotes a marked emotional sensitiveness and often a tendency to affective instability. The frequency of the reactions, therefore, can be taken in a certain measure as indicative of the degree of emotional reactivity.

In this connection two cases may be mentioned which gave numerous spontaneous waves (66, 48):

1. 66 *spontaneous waves.* Worrying, over-conscientious, self-depreciating.—Speaks of feelings of inferiority due to influence of very domineering father.—Marked dislikes.—Affected by emotions of others. —Tries to be consistent under all circumstances and systematic in all activities.
2. 48 *spontaneous waves.* Worrying, restless, over-conscientious.—Some artistic tendencies.—Acts upon impulse.—Dreaming.—Mood variable.—Says he is self-centred, passive, shy, easily embarrassed and easily disappointed.—Complains of 'inferiority complex' and feelings of failure.—Much stirred by moods of others.

From these descriptions we conclude a marked emotional reactivity and an affective unevenness and instability. These individuals are under the domination of moods which may exist quite independent of the momentary environmental situation; an insufficient connection between the galvanic waves and the stimuli is therefore to be expected. Whether the features of restlessness and apprehension present in these cases are generally productive of spontaneous fluctuations cannot be decided from our limited material.

In general, however, we find throughout the entire group of sixty-four students a marked preponderance of direct reactions. This means, in our interpretation, that the affective states of these individuals arise in connection with or on the occasion of environmental stimuli and that they are not of entirely endogenous origin. This is typical for the average normal individual.

Beside the frequency of the various types of responses other features of the curves were examined and compared with features of the emotional make-up as obtained from the personality data.

It was found that the amplitude of the galvanic waves and the occurrence of secondary swings were not indicative of specific emotional characteristics, but were of value rather in the determination of those issues which are especially important for the individual. The reactions of higher amplitude quite naturally indicate a higher emotional value of the respective causative stimuli, while secondary waves which place a similar emphasis on the preceding stimulus are a sign not so much of the depth as of the duration of the affective swing.

In this group of subjects the shapes of both the galvanic and the respiratory curves were found to be predominantly regular and the slight irregularities which did occur could not be linked up with any consistent emotional peculiarity. That higher degrees of irregularity such as are found in pathological cases are significant is shown in the presentation of results from experiments on patients.

Summary.

1. Formal elements of the galvanic curve (type of reactions, frequency, shape) were singled out and linked up with concomitant characteristics of the emotional make-up. By this method the psycho-galvanic reflex phenomenon may be used as an indicator of characteristic features of the emotional make-up of groups of individuals and in a lesser degree of single individuals.

The data were furnished through experiments on sixty-four normal subjects (medical students). As stimuli were chosen: a list of fifty words, sensory stimuli, threats, deep breathing and the suggestion to imagine a situation involving an emotion. The galvanic reactions were obtained without introducing an external current. A record of the respiration was taken during the 30–40 minutes of the experiment.

2. The average number of words reacted to was about one-third of the total list. The reactions were for the most part in close connection with the stimuli. Spontaneous waves occurred in only a few cases in which there was a high emotional sensitivity with a tendency to instability.

3. The comparison of the frequency with the subjects' description of their own emotional make-up gave definite correspondence inasmuch as

the individuals with smallest number of responses were of a well-balanced and not especially sensitive emotional constitution, whereas the cases with most frequent reaction were of a more excitable and labile emotional type.

4. The shape of the galvanic and respiratory curves was in these normal individuals preponderantly regular.

5. Individuals with many galvanic reactions tended to show a low electrical resistance; but low resistance, on the other hand, was not found to be necessarily indicative of a high frequency of responses nor of special emotional characteristics. Variations of the resting current showed no consistent correlation with emotional traits of the individuals experimented on.

It is a pleasure to me to acknowledge my indebtedness to those whose co-operation made this study possible: to the subjects for giving their time and interest; to Dr A. Meyer for the privilege of using the students' personality studies; to Dr C. Richter for his advice concerning technique and procedure; to Dr E. Kinder for assistance and suggestions in working up the material.

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PUBLICATIONS RECENTLY RECEIVED

Feeling Experience and its Modalities. An experimental study by Gerald B. Phelan, M.A., Ph.D. London: Kegan Paul; Louvain: A. Ugstpruyst, 1925.

This study issues from the laboratory of experimental psychology of the University of Louvain and is a welcome addition to the experimental pioneer work that is being done by the impression method. In his experimental arrangements the author follows on a whole that adopted by Wohlgemuth as described in his monograph 'Pleasure-Unpleasure,' but using a much greater variety of stimuli. These stimuli were applied to the subjects who introspected and then dictated the protocol. It is to be regretted that these have not been published here in full, for in this class of work, which depends entirely upon the description of the introspecting observer, it is most desirable that the reader should be enabled to form his own judgments by the perusal of the *entire* protocol. As it is, we are given merely the interpretation of the experimenter backed up by some extracts. How much these interpretations would be affected by the unpublished parts of the protocols, no one can say. Seven observers assisted in this research, of whom only four were trained. 789 experiments were done, 572 by the trained, and 217 by the untrained subjects. There were two series of experiments, namely: (I) 615 experiments where single stimuli were given, and (II) 174 experiments with stimuli in pairs.

What the author exactly means by 'Modalities' of feeling-experience is, at first, anything but clear. Helmholtz, as we know, introduced the conception of modality as distinguished from that of quality into the psychology of sensation. 'Modality' was the generic and 'quality' the specific term. Whilst the perceptions derived from the excitations of a given sense-organ could differ in quality, *e.g.* red and green, a low and a high tone, perceptions from different sense-organs differed in modality, *e.g.* light and sound perceptions. Phelan soon draws a distinction between *objective* and *subjective* feelings, and as he speaks repeatedly of "the quality (pleasant or unpleasant) of the experience," it follows that 'objectivity' and 'subjectivity' are intended to be the modalities, which is at last confirmed on p. 69 where he speaks of the 'fundamental differences' of the affective responses. 'Modality' seems, however, not to appear anywhere in the text, but only in the title of the book.

It will be readily admitted that feelings (pleasure-unpleasure) may be closely connected with visual, auditory, tactile and other sensorial experiences on the one hand, and with kinesthetic experiences, memories, images, etc., on the other. The touch of a powder-puff is pleasant, so are the kinaesthetic experiences derived from yawning, and because our concept of objectivity may in part be derived from experiences of the former sort, whilst experiences of the latter may help in the formation of the concept of subjectivity we may call them, if we choose to do so, objective and subjective experiences (although valid reasons against this terminology may suggest themselves) and the feelings attached to them, objective and subjective feelings respectively. But, surely, there is no *fundamental difference*, as the author suggests, between the feelings in these cases. To turn a difference of attachment of these feelings into *modalities of feeling* is quite unwarranted. One cannot help strongly suspecting that metaphysical considerations influence the author as well as several of his observers. The 'Laboratory atmosphere' makes itself felt. Most independent observers will certainly demur to the statement on p. 86: "...the difference between what we call 'objective' and what we call 'subjective' feelings appears as an immediate datum of experience...." There is not, speaking from memory, a single reference made in Wohlgemuth's 800 published records to such a 'fundamental difference' or 'immediate datum of experience.'

Another point which invites criticism is the use of the terms 'positive and negative feelings' for pleasure and unpleasure respectively. Pleasure and unpleasure are not mutually exclusive, nor is it necessary to pass through a zero-point when a change of feeling-tone takes place, as the terms positive and negative would imply. There is no advantage whatever in this terminology but only the possibility of mistake and mischief. Our author also commits the error of the so-called 'stimulus and meaning errors'; he says (p. 64): "There is no *a priori* reason why we should conclude that an individual feels a pleasant or unpleasant response when he simply states that the stimulus object is pleasant or unpleasant." This may be true enough at the moment when the observer dictates his protocol, but during the experiment of which the protocol gives an account he surely must have *felt* pleasure or unpleasure, or else how could he know that the stimulus was pleasant or unpleasant?

In Chapter IV ("The Condition of Feeling") great industry is evidenced by no less than thirteen tables giving various percentages of responses, etc., but these convey very little. For instance, one table gives a Susceptibility Ranking of the observers in the different series based upon the *number* of responses. This may possibly give an indication for ranking in keenness of observation, but for susceptibility to feeling intensity of the responses is more essential than number of responses. The statement on p. 203: "There is a certain parallelism between the frequency and the intensity of feelings; stimuli which *often* arouse feelings, arouse *strong* feelings," will probably find many critics. This may possibly be true, but it does not follow from the data available here.

The second series of experiments is of greater interest. Stimuli were given in pairs. The author discusses here what he calls the 'Contact' between the responses, and distinguishes four kinds, viz. (1) Isolation, (2) Expulsion, (3) Domination, (4) Coalescence. The absence of published protocols makes itself very much felt here, for on p. 217 the author resorts to what in their absence appears to be a very dangerous procedure. To find causes for 'domination' he refers back to the feeling responses of Series I where the stimuli were given singly which in Series II were given in pairs. But from the extracts of the protocols of Series II it is impossible to see how a quantitative comparison of intensities is feasible. On p. 230 Phelan discusses two cases of 'Recognition of feeling' which by the extracts from the protocols do not appear to be anything of the sort. In the first case the stimuli were the odour of oranges and the sweet taste of sugar, about which the subject reports: "*J'ai l'impression de reconnaître le caractère agréable comme tel. C'est une espèce de sensation de déjà vu, comme une récognition sensorielle.*" To begin with, *le caractère agréable* does not mean *le sentiment agréable*, nor does the context tell us whether the subject refers to the taste or the smell, to the present reviewer's mind he refers to the whole situation, the compound experience, and not merely to its feeling-tones. The incompleteness of the protocol is felt very acutely. The second case is still more definite: "*Cet état m'a paru directement le même que le précédent. C'est l'impression d'une similitude vécue.*" Here it is doubtless a question of a complete situation, of a whole response and not of a feeling-tone only. It seems indeed hardly feasible that there can be any recognition of feelings as such. For they have no memory-images and, quâ feelings, pleasure or unpleasure, there are no differences except intensity and attachment to cognitive or conative processes. This discussion on 'recognition' recalls a criticism by the author on p. 28 of Wohlgemuth's rule XXIX, where Wohlgemuth evidently did not use the term 'recognition' in its strict technical sense, but in the literary sense of making out, discovering, identifying, becoming aware, etc. This criticism of Wohlgemuth is well merited, but then one would expect that the critic would be more precise in his own use of this term. However, we find that Phelan uses it in the same sense as Wohlgemuth on pp. 175, 177, 225, etc.

The author devotes much space to the discussion of the existence of 'pure' feelings and comes to the conclusion that "it seems advisable to accept the hypothesis that feeling is always constitutive of an experience which possesses other aspects as well (sensorial, imaginal, conational, memory aspects and so forth), and, that there are no 'pure' feeling experiences." If by 'pure' he means that there is no other conscious content, then, of course, he is right. But if by 'pure' he means that the feeling does not belong to any of the other contents of consciousness, that the feeling is, so to speak, floating independent of the other coexisting conscious processes, then he is certainly mistaken. Another finding of the author's is also against the evidence of previous research, and not only this but also against his own, *vide supra*, namely, that "it is impossible to establish quantitative comparison between objective feelings. The subjects say that they differ qualitatively from one another, just as the sensorial impressions themselves. When a comparison takes place, it is not a comparison of *feelings* but of *values*" (p. 256).

In an appendix is given an account of a supplementary series of experiments which were preparatory to a further investigation not then finished and to which we may look forward, as they appear to be very interesting.

Above we have mostly advanced points of disagreement and criticism and have hardly touched upon the many points of agreement. We therefore hasten to say that Phelan's work is, without doubt, a valuable contribution to the psychology of feeling, and we are looking forward to the publication of the new research which promises to be still more interesting. If we may venture a suggestion we should like to say that in such difficult introspective research it is advisable to work only with highly trained observers. To experiment with untrained observers is practically waste of time.

A. W.

Problems of Dynamic Psychology. A Critique of Psycho-analysis and Suggested Formulations. By JOHN T. MACCURDY. Cambridge Univ. Press, 1923. Pp. xv + 383. 12*s*. 6*d*.

The purpose of this book is two-fold: (*a*) to show, from demonstration of the limitation and inconsistencies of Freudian formulations, that a broader system is needed; and (*b*) to outline tentative hypotheses to make good this need.

Psycho-analysis is a comparatively young science. It is natural that the theories of its exponents should be undergoing constant modification, and that the disciples of Freud should, with missionary zeal, out-Freud Freud in their teachings. His followers perhaps forget that Freud is often a philosopher, who follows up a line of thought out of scientific curiosity, without, as he himself says on p. 76 of *Beyond the Pleasure Principle*, being of necessity either convinced himself, or seeking to convince others of the truth of the theories. For these reasons a feeling of dissatisfaction with the orthodox psycho-analytic formulations has been growing up, especially since the war neuroses demonstrated clearly the need for broader formulations. This book, therefore, written by a follower of Freud with many years' clinical experience, comes at a most opportune moment. Dispassionate criticism, by those qualified to criticise, is the best way of serving the science.

Dr MacCurdy begins with a closely reasoned criticism of Freud's writings. He then shows the relationship between psycho-analysis and suggestion; and, in Part III, he discusses Auto-erotism, the Origin of Symbols, Burrow's 'Primary Subjective Phase,' and Ferenczi's paper on 'Stages in the Development of the Sense of Reality.' All this is hard reading for anyone who is not well versed in Freudian literature, chiefly because so much has had to be compressed into small compass.

The last part of the book, in which Dr MacCurdy proceeds to outline his own hypotheses is easier reading. He still, however, has one more chapter of criticism, in which he somewhat severely handles the theories expressed by Rivers in his *Instinct and the Unconscious*. There is no doubt that, since Rivers' main work lay outside the boundaries of psycho-pathology, his theories and generalisations may have been based on insufficient clinical evidence; but his quick insight and original mind made almost all that he did of value. This Dr MacCurdy is himself the first to admit when, in the last paragraph of Chapter XVII, he says, "Rivers...has written a book that may prove to be a landmark in the evolution of psycho-pathology."

Finally we come to the constructive portion of the book. Dr MacCurdy begins with Instincts. An Instinct, he defines as "a type of phenomenon which is unreflective, non-discriminative, immediate and uncontrolled in operation, ineradicable and affective," a definition agreeing very closely with that given by Rivers. Instinctive actions appear, in man, only in emergencies; normally man's conduct is ruled by 'instinct-motivations,' a later stage in the evolution of instincts, which Dr MacCurdy traces from the primitive reflex, through the conditioned reflex and the true instincts, to this higher type. Since instincts cannot have free expression in their crude form, their further evolution is necessary. They become unconscious, and form the basis of the 'motivations.' An 'instinct-motivation' is the same as the Freudian 'wish,' a label Dr MacCurdy would discard because it has a conscious connotation. Instincts have no energy of their own; they are the pathways by which the psychical energy, the source of which is not considered, is directed.

Instincts are divided into three groups, each of which is discussed at length in a separate chapter.

The *Ego* instincts come first. They include all those reactions which centre round the 'ego'; not only those of self-preservation. The emotions accompanying these reactions are simple, *e.g.* fear, anger, apathy, or the pure joy which comes with the lust of power. The finer emotions, which have an element of sympathy, have no place. As the child grows up, the instincts take on the form of motivations. These may appear in any of five directions: (1) Bodily pleasures, food, drink; (2) Intellectual curiosity; (3) Lust of power, with its outcome, cruelty and brutality; (4) Desire for recognition, which is allied with (3); and (5) Desire for security, which is opposed to all the other reactions. Most of these motivations require little modification to be given social expression, so they are responsible for comparatively few pathological states.

The second group of instincts is the *Sex* group. These instincts have to do with the propagation of the species, and the search for the pleasures associated therewith. Dr MacCurdy traces the development of the Oedipus complex, the first stage in the formation of the sex-instinct-motivations. This complex he considers "is not only inevitable, but essential" (p. 295). He shows that the normal adult passes through the three stages of narcissism, homosexuality, and heterosexuality—the first and third of which are "essential for normal development" (p. 315),—and briefly indicates the way in which regression may be caused. As to sex enlightenment of children he wisely considers that 'objectivation of interest,' *i.e.* the acquisition of other than purely human interests, is of more importance "than actual and accurate sex instruction" (pp. 306–7).

The *Herd* instincts form the third group. One great difference, between the motivations arising from the herd instincts and those arising from instincts in other groups, is that the herd-instinct-motivations are elaborated by the herd and then adopted by the individual. Their various manifestations, *e.g.* collective thinking, with the consequent lessening of individual responsibility; the antipathy to incest, etc. are discussed in an interesting manner.

The last chapter is devoted to a discussion of the 'Cooperation and Conflicts of Instincts,' the latter figuring most largely. When the conflict is between the sex and ego instincts there is seldom repression: a solution is usually found in alternation of attention. But herd life often requires the "suppression (and annihilation) of purely ego tendencies," although there is, as a rule, ample scope for ego expression. Pathological conditions may arise where the ego is very predominant, or as a result of imprisonment or ostracism. There is then, in general, "regression to that phylogenetic state where the individual exists in a purely hostile environment." The most important conflicts are those between sex and herd instincts. They are of two types according as the propagation impulse of the sex instinct, or the pleasure impulse, is antagonistic to herd standards. The latter type of conflict is the most common cause of psychotic trouble. For conflicts between different kinds of sex ambitions Dr MacCurdy puts forward a simpler explanation than Freud's, on the assumption that the repressive energy is supplied by the herd instincts.

He then considers the causes which precipitate morbid psychological conditions, and shows how the immediate determinants of the symptoms may be sexual, without the general cause of the disease having much to do with sex; in fact few symptoms seem to have a simple relationship to one group of instincts alone. He definitely disagrees with Freud's analysis of 'anxiety.' He elaborates Rivers' hypothesis that one instinct may borrow the reaction elaborated with another, and so explains how most heterogeneous unconscious ideas find expression in a small number of hysterical symptoms; whence it follows that such a symptom will only disappear after prolonged and elaborate analysis.

The work as a whole is a decided contribution to psycho-analytic literature. There are those who will think that Dr MacCurdy still lays too much emphasis on sex; while many Freudians will doubtless feel that he has strayed from the fold. To all students of psycho-analysis *Dynamic Psychology* may be recommended as a book worthy of their careful study and attention. H. B.

Der Moderne Dandy. By OTTO MANN (Heidleberg). Berlin: Verlag Julius Springer, 1925.

The Dandy represented the Artist of the Period; the subject and object of his art being his own proper person. At the time when he made his appearance all other manifestations of art were in abeyance. For reasons explained later, England was his native land, the French subsequently adopted the pose, but in Germany, only the novels of Goethe filled a similar position in the history of her art.

The true Dandy, we read, "is not merely vain, merely seeking after effect, he represents an Idea; he expresses a philosophy, and belongs to two stages of culture and is a significant individual for her recognition." Beau Brummel and Oscar Wilde have been taken as two representative types, the former the founder of the movement, the latter, one of its last exponents. The author adds that the Dandy is the last outburst of heroism in a period of decadence, but points out that both Shelley and Wilde derived much of their inspiration from the study of the Greek classics, and sought to revive in their writings, persons and ideals, the ancient veneration of personal beauty.

The Dandy came into existence first in England, being the outcome of the stoicism and reticence characteristic of the English race, and the calculating cold-bloodedness, which may also be regarded as a distinguishing feature of the Dandy, in addition to his innate vanity and natural gifts of personal beauty and good taste, has been developed occasionally in a criminal direction, producing, for example, the poisoner, who has brought his propensity for murder to the level of a fine art with unprecedented fastidiousness. Dandyism can be looked upon as a play, in which there is but one actor, who plays his part upon the stage of the world and for the term of his whole life. Among the most extreme consequences of the tendency may be counted shamelessness and crime.

Biological Memory. By EUGENIO RIGNANO. Translated and with an Introduction by Prof. E. W. MACBRIDE. London: Kegan Paul, Trench, Trübner and Co., Ltd., 1925. 10*s.* 6*d.* net.

Prof. MacBride has produced an admirable translation of a very stimulating book. On p. 2 of his introduction he writes "If Prof. Rignano really had been able to elaborate a theory of life which was complete and satisfactory in all its details, he would have succeeded in solving one of the main problems of our existence." We feel sure that the great majority of readers will agree with this, but we fear that they will also agree with its implication, that Prof. Rignano has not done so.

The author opens his book with a discussion of some of the age old biological problems,—"the three dilemmas of Embryology"—and proceeds to show that they have not been solved as yet, but that they could be solved by a certain hypothesis of the author's, called Centro-Epigenesis. As we proceed with the text we find that the life-history of an organism consists in the accumulation in its 'elementary accumulators' of 'nervous energies' appropriate to all the influences of its environment upon it. Every stimulus sets up the passage of a characteristic energy current, which in its turn deposits a characteristic accumulation in the right place at the right time. Images, habits, and other mnemonic phenomena are the result of further stimuli being appropriate enough to convert these accumulations back into the original energy current.

By elaborating this hypothesis Prof. Rignano claims to be able to explain all the apparent peculiarities of living matter, and yet to avoid the vitatistic Scylla and the mechanistic Charybdis. But it is difficult to understand why the correct elementary accumulators should accumulate the necessary kinds of energy, and on what system the subsequent release of these energies is organised, so that the right type of development is set about. It was to explain this that Entelechy was invented.

The writer gives an excellent summary of Semon's work, and proceeds to develop a psychology of the 'Affective Tendencies.' These are functional processes directed to the re-establishment of necessary physiological states, or to the creation of new possible states, which are adaptations. Thus the affective tendency which acts in the first case as a *vis a fronte* becomes a mnemonic property of the organism and in all subsequent cases acts as a *vis a tergo* by proxy. It seems difficult to understand how these affective tendencies arise in the first place upon a mnemonic basis. Prof. Rignano is not strikingly clear on this point, and his theory of the evolution of sex, with all its complexity and subtlety of organisation, from a need to eliminate waste products, is not very illuminating.

Perhaps the most interesting part of the book is Botazzi's criticism of Rignano, and the latter's reply, which is largely a very justifiable criticism of mechanistic biology.

The remaining psychological chapters are rather schematic, but will be found very interesting.

The weakness of the book lies in the attempt to explain any developmental or creative process in terms of a concatenation of traces, for the latter can never lead to anything but stagnation. Prof. Rignano slurs over this important problem—"Just what is it that gives rise to an adaptive development?" He will not allow himself a mechanist and so he cannot regard all adaptation as a passive process; and yet he would explain vitatism in terms of mere record, or memory. It would seem on his own showing that to explain life in terms of Memory is just what cannot be done.

R. W. P.

Industrial Psychology in Great Britain. By Dr C. S. MYERS. London: Jonathan Cape, Ltd., 1925. Pp. 164. 7*s.* 6*d.* net.

This is the best brief account of the application of psychology to industry that has yet been published. Having regard to the very large number of books on Industrial Psychology which have appeared during the last few years such a statement may seem somewhat sweeping. But it can be made without fear and without reserve. Anyone who has worked really seriously with Dr Myers' *Text Book of Experimental Psychology* knows that he possesses extraordinary powers of concise and clear exposition, and an equally remarkable capacity for hitting upon precisely the facts of real importance in a vast mass of detailed and specialised work. These qualities are strikingly illustrated again in this book. Particularly the many people who say that industrial psychology is not really 'psychology' at all, but only a kind of common sense mostly about small mechanical matters, ought to read the lectures. There is throughout an insistence upon the part which systematic laboratory investigation must play if industrial psychology is to be set upon a sound basis.

The topics dealt with are familiar ones. First an account is given of the organisation of industrial psychology in Great Britain, and then Industrial Fatigue, Movement Study, Vocational Selection and Vocational Guidance are discussed in turn. But though the topics may be familiar the treatment is fresh, the illustrations and examples new and at first hand, and the conclusions are stated without exaggeration and in a most convincing manner.

Pleasure and Pain. A Theory of the Energic Foundation of Feeling. By PAUL BOUSFIELD. London: Kegan Paul, 1925. Pp. 114. 4*s.* 6*d.* net.

Dr Bousfield's theory of pleasure and pain is, briefly, that all organisms are in a state of 'tension,' that the pain or unpleasure felt at any moment is (roughly) proportional to the amount of 'tension' present, and that the pleasure felt is proportional to the rate of decrease of the 'tension.' But unfortunately the author does not give us much idea of what he means by 'tension,' or indicate satisfactorily how we can judge the amount of 'tension' present under any given conditions—except, of course, by assuming the truth of the theory. Hence Dr Bousfield has not succeeded in proving his case, though we are far from suggesting that his theory is altogether false.

A feature of the book which makes for clearness is the use of graphs to show how the 'tension' is supposed to vary in certain cases. If their hypothetical nature is kept clearly in mind such graphs may serve a useful purpose. But the meanings given to some of the symbols in the algebraic equations are very vague, and there is a slight mistake (on p. 32) about the conditions under which a differential coefficient becomes infinite.

Psychology for Nurses: Introductory Lectures for Nurses upon Psychology and Psycho Analysis. By MARY CHADWICK. London: William Heinemann (Medical Books), Ltd., 1925. Pp. xvi + 249. 6*s.* net.

This little book is interesting, clearly and attractively written, and should prove extremely useful to those for whom it is intended. It might indeed be better if, before she is plunged into the doctrine of psychoanalysis, the ordinary nurse were given some training in general psychological method and theory. The author of this book has comparatively little to say about other methods and other points of view than those of Freud. But no doubt this is exactly what she intended, and within her limits she has done her work well. It should be impossible for any nurse to read this book intelligently without gaining both increased interest in her work, and greater efficiency in dealing with her daily problems in so far as these have to do with mental life.

Die Philosophie in ihren Einzelgebieten. Herausgegeben von MAX DESSOIR. Berlin: Ullstein, 1925. Pp. 958.

This immense volume contains articles as follows: on Logic by J. Baptist Rieffert; on Theory of Knowledge and Metaphysics by Erich Becher; on *Naturphilosophie* by Moritz Schlick; on Psychology by Kurt Koffka; on Aesthetics and the Philosophy of Art by Emil Ukitz; on Ethics by Paul Menzer; on Philosophy of Religion by Paul Tillich; and on Social Philosophy and the Philosophy of History by Alfred Vierkandt.

The Ego and Physical Force. By J. C. ISBYAM. London: The C. W. Daniel Co., 1925. Pp. viii + 138. 5*s.* net.

The recent advances in physics and psychology—particularly the quantum theory and psychoanalysis—have, it is urged, made it extremely difficult to "think consistently about science." The author therefore presents a series of metaphysical arguments, several of them in dialogue form, which are claimed to overcome these difficulties.

Mind: its Origin and Goal. By Dr GEORGE BARTON CUTTEN. New Haven: Yale University Press (London: Humphrey Milford), 1925. Pp. xiii + 213. 11*s.* 6*d.* net.

Dr Cutten writes for the general reader upon a number of much discussed topics of which some are: Where did we get our minds? Do we have animal mind? Is mental development like that of biology and history? What are the relations of Intelligence and Instinct? What future changes may we expect? The book is easy and pleasant to read, but not very original. It was published by the James Wesley Cooper Memorial Publication Fund.

Telepathy and Clairvoyance. By RUDOLF TISCHNER. International Library of Psychology, Philosophy and Scientific Method. London: Kegan Paul, 1925. Pp. xi + 227. 10*s.* 6*d.* net.

The author of this book began to be actively interested in "supernormal phenomena" in 1912. Since then he has carried out a large number of experimental observations upon telepathy and clairvoyance. The account which he gives of these is clear and concise. They lead him to a theory which is based in principle on the possibility of direct communication from one mental life to another apart from bodily cooperation. Whether the theory is accepted or not the observations recorded are certainly of great interest. The translation, by W. D. Hutchinson, reads well.

Our Minds and their Bodies. By Professor JOHN LAIRD. The World's Manuals. London: Oxford University Press, 1925. Pp. 122. 2*s.* 6*d.* net.

Professor Laird has written a bright and interesting small book on the problems of the relations of body and mind. The greater part of this, and probably the best part, is written from the standpoint of the philosopher and metaphysician. There is however a section dealing with "The Evidence of the Sciences," and this is divided into two parts, one of which deals with "The Psychological Aspect," while the other approaches the subject by references to the works of physiologists and biologists.

More Light: a simple account of how the Brain works. By S. CAMERON, B.A. London: Williams and Norgate, Ltd., 1925. Pp. xiii + 75. 3*s.* 6*d.* net.

This account is not so simple as the author believes. He appears to treat the central nervous system merely as a constantly active source of energy. The energy spreads out to the peripheral sense organs and they, somehow, do the rest. There are no afferent sensations, and the whole physiology of reflex action is said to be mythical. On this basis the author valiantly endeavours to explain cognition, volition, memory, the birth of souls, and God. He includes a portrait of himself and fortunately writes clearly and concisely.

The Dawn of Character in the Child. By EDITH E. READ MUMFORD London: Longmans Green and Co., 1925. Pp. xv + 242. 5*s.* net.

The subject matter of the new edition of this well-known and popular book has been considerably re-arranged and some new material has been added. The book is undoubtedly sensible and helpful. It is based largely on first-hand study. That it suggests that the 'dawn of character' is a more regular and orderly affair than is generally the case is perhaps inevitable.

Consider the Child. By MARY EATON. London: Longmans Green and Co., 1925. Pp. vii + 256. 4*s.* 6*d.* net.

This book is addressed to 'students in training,' who are to work through it as they would work through "a set of exercises in Mathematics or any other subject." The writing is disconcertingly scrappy, there are multitudes of questions, many of them ingenious, but most of the type easier to ask than to answer, and the total effect of the book is rather bewildering. The author's intention is to foster an independent attitude of mind towards the problems of pedagogy.

Education as the Psychologist sees it. By Professor W. B. TILLSBURY. New York: The Macmillan Co., 1925. Pp. ix + 342. 8*s.* 6*d.* net.

This is a sober, well-informed but by no means exciting treatise on the main points of contact between modern psychology and education. It is exactly what it professes to be, *i.e.* a text book for the educational student, and not an original contribution to the psychology of education.

Education as World-Building. By THOMAS DAVIDSON. Edited with an Introduction by ERNEST MOORE. Cambridge, U.S.A.: Harvard University Press (London: Humphrey Milford), 1925. Pp. xxxiv + 59. 6*s*. net.

Mr Davidson's aim is to show that true education consists in the development in the individual of an ordered world of thought, feeling and action so that he can take a harmonious place in society. Neither the thesis nor the treatment of it is novel, but the essay is pleasant enough to read. Dr Moore contributes a biographical introduction.

Elementary Psychology. By Professor ARTHUR I. GATES. New York: The Macmillan Co., 1925. Pp. xiv + 594. 12*s*. net.

This is quite a good introduction to the study of psychology, but it is hard to see that it is any better than a good many others. Professor Gates deals first with the physiological mechanisms underlying behaviour, then with problems of conduct belonging to the reactive and affective side, and last with the more intellectual processes, and with questions of mental organisation as a whole.

Experimental Psychology and Education. By Professor C. W. VALENTINE. London: W. B. Clive. Second Edition, 1926. Pp. x + 240. 4*s*.

For the student working mainly by himself and interested in many of the problems of experimental psychology there is probably no better book than this. The second edition has been considerably enlarged and contains new sections on the Psychology of Thought Processes, The Growth of Concepts, The Speed of Reading, Tests of Practical Ability, The Appreciation of Pictures and of Poetry, and the Transfer of Training. Many of the old sections have been enlarged.

The Brain and the Mind. By PAUL SÜNNER. Trans. by C. H. BROOKS. London: George Allen and Unwin, Ltd., 1926. Pp. 112. 3*s*. 6*d*. net.

This is a popular account, partly historical and partly critical, of views regarding the relation of brain and mind. It is written with apparent clarity and with knowledge. But there are a bewildering number of references to various authorities. The translation is admirable.

The Theories of Instinct. By Professor E. C. WILM. New Haven: Yale University Press (London: Humphrey Milford), 1925. Pp. xiv + 188. 11*s*. 6*d*. net.

Professor Wilm has written a history of theories of instinct from the pre-Socratics to Darwin and Weismann. His account is clear, but of necessity very brief. It is rather a pity that he finishes just when views about instinct are ceasing to be mainly philosophical in their bearing and are becoming more biological and psychological. Still the book fills a gap, and fills it very well, so that it should be well received.

The Purpose of Education, an Examination of Educational Problems in the Light of Recent Scientific Research. By St GEORGE LANE FOX-PITT. Fifth issue, revised. Cambridge University Press, 1925. Pp. xxix + 94. 4*s*. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING

(WITH THE AESTHETICS SECTION).

May 27, 1926. The Creative Process in the Artist's Mind. By Prof. S. ALEXANDER.

June 26, 1926. The Passing of the Psychology of 'Shapes' (Gestalten) into that of 'Wholes' (Ganzheiten). By Prof. C. SPEARMAN.

Modern Theories of the Self. By REX KNIGHT.

SECTIONAL MEETINGS.

AESTHETICS SECTION.

May 27, 1926. As above.

EDUCATION SECTION.

April 19, 1926. Number Games: their Value as Drill in Arithmetic. By A. J. HUGHES.

June 14, 1926. The Correlation between Size of Family and Intelligence of the Children. By H. E. G. SUTHERLAND.

MEDICAL SECTION.

April 28, 1926. What is a Mental Illness? By Dr N. H. M. BURKE.

EIGHTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

SEPTEMBER 6TH—11TH 1926

THIRD ANNOUNCEMENT

The following further contributions have been promised:-

Understanding and Explanation: Erismann, Bonn; Ewald, Erlangen.

Shape-qualities: Rubin, Copenhagen; Sander, Leipsic.

Religious Psychology: E. Jones, London; Thouless, Manchester.

In addition to the names, mentioned in our former announcement, of those prepared to give separate addresses, we can give the following: Ach, Göttingen; Adler, Vienna; Aveling, London; Buysse, Louvain; Ettlinger, Münster; Godefroy, Amsterdam; Grünbaum, Amsterdam; Henning, Danzig; Herbertz, Thun; F. B. Hofmann, Berlin; Jaederholm, Göteborg; Klemm, Leipsic; Lahy, Paris; Langfeld, Princeton; Lewin, Berlin; Linde, Göteborg; Lipmann, Berlin; Minkowski, Paris; O'Rourke, Washington; Pfister, Zurich; Pfungst, Frankfort-on-Main; Poppelreuter, Bonn; Sjöbring, Uppsala; Thouless, Manchester; Wertheimer, Berlin; Wimmer, Copenhagen; Wreschner, Zürich; Wynn Jones, Leeds.

Sections will be formed for the discussion of Eidetic imagery, Psychogalvanic reflex, Higher psychic processes, Animal Psychology, Psychopathology and Applied Psychology.

The International Association for Psychology and Techno-Psychology (*i.e* Industrial Psychology) began its official activity on March 15th Fifteen European countries are represented in the Association, whose administrative headquarters are at Riga, under the direction of Dr Moeller. In England, the Association is represented by Professor T. H. Pear, University of Manchester, and its department of Techno-Psychology by Dr C. S. Myers, National Institute of Industrial Psychology. The first publication of the Association will be *A Survey of the Organisation and Position of Techno-Psychology.*

VOLUME XVII OCTOBER, 1926 No. 2

THE CORRELATION BETWEEN INTELLIGENCE AND SIZE OF FAMILY[1]

H. E. G. SUTHERLAND AND GODFREY H. THOMSON.
(*From Moray House, University of Edinburgh.*)

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1. *Introduction* (pp. 81, 82).
2. *Subjects and Data* (pp. 82–84).
3. *The Results with the Unselected Group* (p. 84).
4. *Influence of Position in Family* (pp. 84–87).
5. *Problem of Unfinished Families* (pp. 87, 88).
6. *First Conclusions* (pp. 88, 89).
7. *Other Groups* (pp. 89–91).
8. *Conclusion* (p. 91).
Appendix (p. 92).

1. INTRODUCTION.

THE object of the present paper is to add to our data regarding the question whether the members of small families are, on the average, more intelligent than the members of large families. The incentive to investigate this problem arises mainly from a fear that our present social conditions are tending to breed intelligence out of the race.

Thomson, in conjunction with J. F. Duff, has already enquired into the correlation between the parents' occupation and the intelligence of the children, and found r approaching 0·30. This has been confirmed with other data by Hector Macdonald under Thomson's guidance, and independent enquiries in fair accord with this are reported[2].

E. J. Bradford[3] last year set out very ably the general problem, in addition to contributing data, to which we shall refer presently, bearing on our special question. He quotes the British census returns for 1921 as showing that the size of family decreases as the parent's occupation tends away from the 'labouring' end of the spectrum of occupational categories and towards the 'professional' end. If we may take the occupational status as some indication of the parent's average I.Q., and if intelligence of parents and children is correlated (as we know it to be) we would expect therefore members of large families to be less intelligent than members of small families. Bradford contributes to this problem a correlation table comprising 393 boys and girls showing $r = -\cdot 25 \pm \cdot 03$ between size of family and Otis Intelligence Test score. The average intelligence score decreases steadily from 24·1 for families of 1 and 2, to 13·9 for families of 9 and over. For 390 children (presumably the same individuals less 3) he finds the average size of family increasing from 2·6 up to 4·4 as the Otis score decreases from 45 down to 5. His children were aged 10, and he points out the difficulty raised by unfinished families to which we shall refer in our own experiment.

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[1] A paper read by Mr Sutherland at a meeting of the Education Section of the British Psychological Society on June 14, 1926. That part of the paper due to Mr Sutherland formed a thesis presented as a portion of the work required for the degree of B.Ed., Edinburgh.

[2] Duff and Thomson, this *Journal*, 1923, XIV, 192; Macdonald, *ibid.* 1925, XVI, 123; Sandiford, *School and Society*, 1926, XXIII, 117; Haggerty and Nash, *J. Educ. Psychol.* 1924, XV, 559.

[3] "Can Present Scholastic Standards be Maintained?" *Forum of Education*, 1925, III, 186.

Terman, in his book *Genetic Studies of Genius* (vol. I), selected, out of 578 families yielding gifted subjects, 92 families which could be safely reckoned as completed, the mother being over 45. Within this highly selected group he found the correlation between size of family and I.Q. to be $-\cdot 271 \pm \cdot 062$, or after making certain allowances for the schooling of the parent, $-\cdot 246 \pm \cdot 063$.

Professor Karl Pearson and Miss Margaret Moul[1] find, on the other hand, no significant correlation between size of family and I.Q., or between position in family and I.Q. Their subjects were 1200 pupils aged 7 to 15 from the Jews Free School, Aldgate, and the measure of intelligence was a controlled estimate on the Biometric Laboratory Scale.

2. SUBJECTS AND DATA.

Our subjects were (1) 1924 elementary school children of the Isle of Wight who at the time of taking an intelligence test were between 10½ and 11½ years old. (1 *a*) 1084 of them were examined in February, 1926, and comprised all the children who were of this age range[2] in the elementary schools, excepting those absent on that day. (1 *b*) The remaining 840 were similarly tested in February, 1925. They are not so unselected as the 1084, for a number of the cleverest had gone off to secondary schools

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[1] *Annals of Eugenics*, 1925, I.

[2] A small number (164) of the 1084 cases were, as we have since discovered, cases which had escaped the 1925 test through illness, etc., and were thus tested when between 11½ and 12½.

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before we, in 1926, obtained the information about size of family. For all these children we also had the information as to position in family[1].

The I.Q. in these cases was provided by a group intelligence test, lasting 45 minutes. These tests were different in 1925 and 1926 and were specially prepared and standardized. The 1925 test was very satisfactory. Its standardization was carried out on some hundreds of children in the north of England, and its norms have since then justified themselves with only slight modifications in other districts. The 1926 test was less satisfactory. Very little time was allowed for its standardization and its norms appear to be too strict in the lower reaches though satisfactory above 110 I.Q. This ought not seriously to affect its reliability for the present purpose however since the age range was narrow.

(2) 386 boys of the Royal Grammar School, Newcastle-on-Tyne, of ages varying from 10 years 8 months to 17 years 4 months. In this instance the I.Q.'s were very reliable, having been measured both in 1922 and in 1923 by the Northumberland Mental Test No. 2, administered by Mr Laws, the senior science master, who is skilled in intelligence testing. The average I.Q. was taken, and ages and families are as in 1923. Position in family was also known. Admission to the school is by examination.

(3) 395 boys and girls of Moray House School, Edinburgh, ranging in age from 7 years 6 months to 15 years 11 months. Their I.Q.'s were measured by a later and improved form of that test used in the Isle of Wight in 1925, but such a test is unsuitable for the youngest and oldest of these children and the results are not therefore very reliable towards the ends of the age scale. Moray House School is the demonstration school of Moray House, the Training College of the Edinburgh Provincial Committee, and the *locus* of the Department of Education of Edinburgh University. The school is an elementary school with an 'advanced division' at its upper end, *i.e.* a kind of junior secondary school, admission to which is by qualifying examination. Even to the elementary division admission is in a certain measure selective, as there are many more candidates, from all over the city, than vacancies. There are no fees, but books have to be purchased.

(4) 30 boys from Ryde Grammar School in the Isle of Wight, of ages

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[1] We are much indebted to the Isle of Wight teachers for their assistance, and to Mr H. Jervis, the Director of Education, for permission to obtain and use these facts. We are similarly indebted to Mr E. R. Thomas, the headmaster, and Mr A. R. Laws, senior science master, for the Newcastle Royal Grammar School data. In all cases the children supplying the information probably ignored siblings who had died in early infancy.

from 10 years 5 months to 12 years 11 months tested by the 1926 Isle of Wight Test.

3. THE RESULTS WITH THE UNSELECTED GROUP.

The following tables give the correlation of intelligence and size of family in the case of the 840 and the 1084 elementary school children from the Isle of Wight[1]. The absolute values of I.Q. are not given, for no stress whatever is laid on their accuracy except relatively to one another. The category letters *a*, *b*, *c*, etc., do not necessarily have the same absolute meaning in the two different sets of data. The correlations are product-moment calculations, assuming that the intelligence categories are quantitatively equidistant. The marginal distributions show that this is not the case, but this error will not, we think, seriously affect our conclusions.

TABLE I.

Categories of I.Q.	Size of family											Totals
	1	2	3	4	5	6	7	8	9	10	Over	
(*a*) (high)	1	10	3	3	3	1	1	—	1	1	1	25
(*b*)	8	8	9	2	6	—	2	1	—	—	1	37
(*c*)	12	14	11	15	10	8	3	1	1	—	—	75
(*d*)	12	24	25	24	18	11	6	9	4	4	2	139
(*e*)	14	29	29	21	18	16	9	7	3	1	1	148
(*f*)	13	24	28	34	18	20	17	16	6	5	9	190
(*g*) (low)	17	26	39	34	34	26	21	5	20	3	1	226
Totals	77	135	144	133	107	82	59	39	35	14	15	840

$r = -\cdot 154 \pm \cdot 023.$

A corresponding table for the 1084 cases of 1926, which can be formed from the Appendix, gives in that case $r = -\cdot 218 \pm \cdot 019$.

The simplest and most striking way in which to show these negative correlations is by Fig. 1.

4. INFLUENCE OF POSITION IN FAMILY.

It is of interest to know also what the influence of position in family may be, whether the eldest born is more intelligent or less: and this factor if present might have a bearing on the phenomenon found in the previous section. This point was investigated in the case of the 1084 group.

If for them a correlation table is formed for position and I.Q. from the data in the Appendix it gives the value $r = -\cdot 200 \pm \cdot 019$.

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[1] The table for the 1084 cases can be assembled from the Appendix and is therefore not printed here.

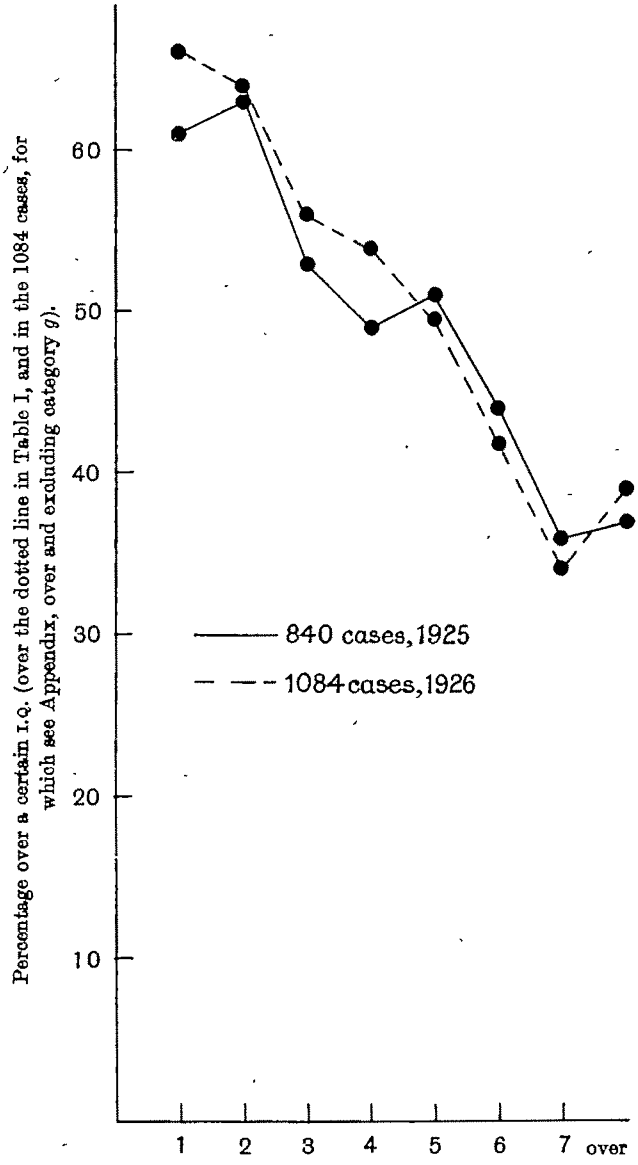

Fig. 1. Decrease of Intelligence with Size of Family.

This r of $-\cdot 2$ is of course largely due, not to position in family, but to size of family: at least the two are here intermingled, for the first born included all the only children, while the seventh born necessarily belong to a large family. One's thoughts naturally turn to the possibility of enquiring, by means of partial correlation, into the separate influences of these two factors. But the difficulty of interpreting partial correlation coefficients, always great, is peculiarly so in this instance, as will become clear shortly. The 'correlation' between size of family and position in family, if one goes through the process of making a table, is as follows:

TABLE II.

Size of family	Position in family								Totals
	1st	2nd	3rd	4th	5th	6th	7th	Over	
1	115	—	—	—	—	—	—	—	115
2	128	84	—	—	—	—	—	—	212
3	52	77	56	—	—	—	—	—	185
4	40	37	39	36	—	—	—	—	152
5	17	13	34	39	24	—	—	—	127
6	9	20	16	20	21	17	—	—	103
7	4	8	19	14	12	8	23	—	88
8	2	2	5	10	11	9	7	4	50
9	—	—	1	2	4	4	6	10	27
10	—	—	—	—	3	4	1	8	16
Over	—	—	—	1	1	1	—	6	9
Totals	367	241	170	122	76	43	37	28	1084

$r = +{\cdot}794 \pm {\cdot}008.$

But this is a very unusual kind of correlation table. One half of it is necessarily empty, for the fourth born *cannot* belong to a family of 3. It is extremely skew. Moreover, if we now proceed to find r for 'constant position in family,' between I.Q. and size, what does this mean? An ordinary partial correlation coefficient, where the three variables are each normally distributed, gives the correlation for *any* constant value of the chosen fixed variable. Here, if we fix the position in family, we seriously limit the *possibilities* and not merely the probabilities. The family *cannot* be less than the fixed position. It is therefore extremely difficult to say what such partial r's may mean. However, if the usual computation is carried out, the result is as follows:

I.Q. and position, for constant size, $-{\cdot}0464$.

I.Q. and size, for constant position, $-{\cdot}0993$.

The actual correlation tables for families of fixed size were formed[1], and calculations in four cases gave the following *experimental* partial correlations directly:

Families of	r of I.Q. with position	Cases
4	$+{\cdot}132 \pm {\cdot}06$	152
5	$-{\cdot}158 \pm {\cdot}06$	127
6	$-{\cdot}013 \pm {\cdot}07$	103
8	$+{\cdot}064 \pm {\cdot}10$	50

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[1] They are indeed the successive strata of the data as given in the Appendix.

These small and irregular results, however, are entangled with the question of unfinished families, to which we now turn.

5. PROBLEM OF UNFINISHED FAMILIES.

All the children in this group of 1084 were approximately 11 years of age. Many of them therefore would belong to families still unfinished. This is especially likely to be the case where the child is in an early position in the family. For example, if one of the above 1084 children is the first of 4, it is quite likely that he will ultimately belong, not to a completed family of 4, but to one of 5, 6 or more. He may therefore be out of place in the correlation tables. The first of the above experimental partial correlations which we calculated was that for families of 4 giving $r = + \cdot 132 \pm \cdot 06$. This value, small as it is compared with its probable error, suggests a correlation such that the eldest born is dullest, a result so unexpected that it occurred to us that unfinished families might explain it: for where all the subjects tested are aged 11, those who are fourth born of 4 almost certainly belong to finished families, whereas those who are first born of 4 possibly do not. All the 152 children who were members of families of 4 gave 53·9 per cent. of their number over a certain level of intelligence (that level previously used), while the 36 of them who were youngest of 4 gave 72·2 per cent., which lent colour to our suspicion. However, the negative correlation in families of 5 between intelligence and position showed that probably mere random differences were the explanation. Still, it is clear that a partial correlation here is very difficult to interpret.

The other partial correlation, between size of family and intelligence, for constant position, is still more difficult to interpret. Fixing any position except the first will simultaneously limit the sizes of family available, for a child cannot be fourth in a family of 1, 2, or 3. If we take all the 367 first born children (first born as far as we know) from the Appendix we get a correlation table which gives $r = - \cdot 102 \pm \cdot 04$. This, so far as it goes, is consistent with the calculated partial correlation found by the formula from the three total correlations, viz. $- \cdot 0993$. But it is clear that in this table the number of small families is selectively increased proportional to the total. And the unfinished family difficulty is extremely acute when all the children are first borns and aged 11.

We have concluded that with the peculiarly interrelated variables here present we are unable to interpret the meaning of partial correlations. But the impression left in our minds is that the negative correlations found are not due to any extent, if at all, to position in the family but to the size of the family. By 'due to the size of the family' we mean causally connected with it in some way. The causal connection is, we fear, that intelligent parents tend to have small families. Whether some other explanation (as that small families have better opportunities for home study) can be advanced is a question we shall leave out of this paper[1].

We proceeded next to eliminate from the 1084 children those in whose cases the probability of belonging to uncompleted families was highest, viz. the first of 4, first and second of 5, first, second, and third of 6. Where there are only two younger children the family is perhaps fairly likely to be finished. When the family is already 7 in number it is large anyhow.

The remaining table, with 968 cases[2], showed an increased correlation of $r = -\cdot 246 \pm \cdot 021$. But further comparison of the children removed by the above rule (who were on the whole rather *above* average intelligence) convinced us again that the change was accidental, although in the expected direction. Finally we made from the Appendix data a table composed wholly of the 371 children (aged 11) who had no younger sibs, and obtained the value $r = -\cdot 177 \pm \cdot 04$. This is possibly the result most free from objection on the score of unfinished families, for where the youngest child is aged 11 it is not very probable that more will be born.

6. FIRST CONCLUSIONS.

Our general impression from all this is that there is no clear proof of any correlation between intelligence and position in family: but that there is a correlation of about $-\cdot 2$ between intelligence and size of family.

Prof. Pearson and Miss Moul found no such correlation among Jewish families. But those families averaged twice as large as ours, and it is clear that among them hardly any restriction of size, or at least much less restriction, was being practised[3]. J. C. Chapman and D. M. Wiggins among American children found with 650 cases $r = -\cdot 33$[4]. That some correlation exists is we think shown by our other data, to which we now turn, though it appears not directly but indirectly.

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[1] It was suggested in the discussion on June 14, by Dr Crichton Miller, that the influence of size of family might arise through the age of the mother. In that case further enquiry into the influence of position in family might be of interest, if the questions of size and position can be disentangled.

[2] It can also, of course, be deduced from the Appendix.

[3] *Annals of Eugenics*, 1925, vol. I.

[4] *Pedagogical Seminary and Journal of Genetic Psychology*, 1925, p. 414.

Before leaving the wider data we would like to emphasize our knowledge of its shortcomings. Especially the very far from normal distribution among the categories of intelligence shows these to be of varying and arbitrary width. Our measure of intelligence was, however, very satisfactory in one important respect, namely that it was uncorrelated with age. The I.Q. technique was used and this technique will, when carefully carried out with children not too near the age of 16 years, give measures independent of age, and moreover the age range here is very narrow. Of the 1084 cases 920 were within six months of their 11th birthday—the remaining 164 children being a year older. (We would have excluded these had we known of their presence at an early enough stage of the calculations.) For these 920 the correlation of I.Q. with age, taken month by month over the 12 months, was only $r = + \cdot 03$, as is shown in Table III.

TABLE III. *Isle of Wight children* (920 *of the* 1084).
Absence of Correlation of I.Q. *with age.*

yr. m.	Categories of intelligence *j*−	*j*	*i*	*h*	*g*	*f*	*e*	*d*	*c*	*b*	*a*	*a*+	Totals
11 11	12	3	7	6	7	17	8	16	9	13	4	4	106
11 10	10	5	7	5	2	10	16	12	4	8	2	1	82
11 9	10	6	7	6	8	9	5	5	7	8	5	1	77
11 8	13	4	—	6	3	5	8	8	5	2	4	2	60
11 7	9	6	7	4	7	9	8	9	12	5	2	3	81
11 6	16	2	6	7	12	7	5	3	6	7	3	1	75
11 5	17	3	10	9	5	11	8	10	4	14	6	1	98
11 4	14	6	8	10	6	4	3	9	9	5	3	1	78
11 3	11	3	7	3	6	5	5	6	5	5	7	2	65
11 2	11	1	4	9	9	3	11	6	2	5	10	2	73
11 1	12	5	5	5	6	7	6	8	1	3	8	—	66
11 0	12	2	5	3	3	5	2	5	5	5	8	4	59
Totals	147	46	73	73	74	92	85	97	69	80	62	22	920

$r = - \cdot 03 \pm \cdot 02.$

7. OTHER GROUPS.

The other groups—386 from Newcastle Royal Grammar School, 395 from Moray House School, and 30 from Ryde Grammar School—differ from the data already considered in being selected for intelligence, both directly by competitive entrance to these schools and indirectly by social position, the two factors probably playing a different part in the three cases, but always tending to select intelligence and reject dullness.

In none of these cases do we find any significant correlation between size of family and intelligence, and we suggest that in the main this is due to the selection for intelligence which has already gone on. The

Ryde numbers are too few to form a useful table: the correlation tables for the other groups are as follows, Tables IV and V.

TABLE IV. *Royal Grammar School, Newcastle-on-Tyne.*
(*Northumberland No.* 2 *Test, twice given.*)

I.Q.	Size of family 1	2	3	4	5	6	7	8	10	Totals
145	—	—	—	1	—	—	—	—	—	1
140	—	—	2	—	—	—	—	—	—	2
135	3	2	4	1	1	—	—	—	—	11
130	4	6	5	4	3	1	—	—	—	23
125	2	9	7	5	—	1	—	—	—	24
120	6	11	8	5	4	1	—	—	1	36
115	4	14	11	10	6	3	1	—	—	49
110	12	14	18	12	2	3	1	—	—	62
105	11	23	14	15	4	2	1	2	5	77
100	7	15	13	7	3	3	1	—	1	50
95	7	10	5	5	1	2	—	1	—	31
90	3	3	3	2	2	—	—	1	—	14
85	—	1	1	—	1	1	—	—	—	4
80	1	—	—	—	—	—	—	1	—	2
Totals	60	108	91	67	27	17	4	5	7	386

$r = -\ 058 \pm \cdot 04.$

TABLE V. *Moray House School.*

Categories of I Q.	Size of family 1	2	3	4	5	6	7	8	9	Totals
(*a*)	—	1	—	—	—	—	—	—	—	1
(*b*)	4	3	4	2	—	1	1	—	—	15
(*c*)	3	10	11	8	3	3	1	—	—	39
(*d*)	15	37	33	17	19	3	3	2	1	130
(*e*)	14	25	45	20	11	5	4	2	—	126
(*f*)	7	14	18	10	4	8	2	—	1	64
(*g*)	2	2	7	4	1	—	1	—	—	17
(*h*)	—	—	—	2	—	—	1	—	—	3
Totals	45	92	118	63	38	20	13	4	2	395

$r = -\cdot 075 \pm \cdot 034.$

Lest in this Moray House Group the unsuitability of the test for the extreme ages might be an explanation we isolated the 131 cases between ages 10¼ and 12¼ and found still only $r = -\ \cdot 056 \pm \cdot 09$.

If this absence of correlation in these samples between intelligence and size of family is as we think due to these children being, by one means or another, selected for intelligence, and if there is, as our Isle of Wight data led us to think, such a correlation in the total, unselected, population, then it would follow that the families in these selected schools ought to be smaller. And this is the actual case. Fig. 2 shows the distribution of families in the three instances, and brings out very clearly the differences. For clearness dots, etc., joined by lines, have been used in Fig. 2 instead of the usual rectangles of a histogram, which do not readily permit of being superimposed without confusion.

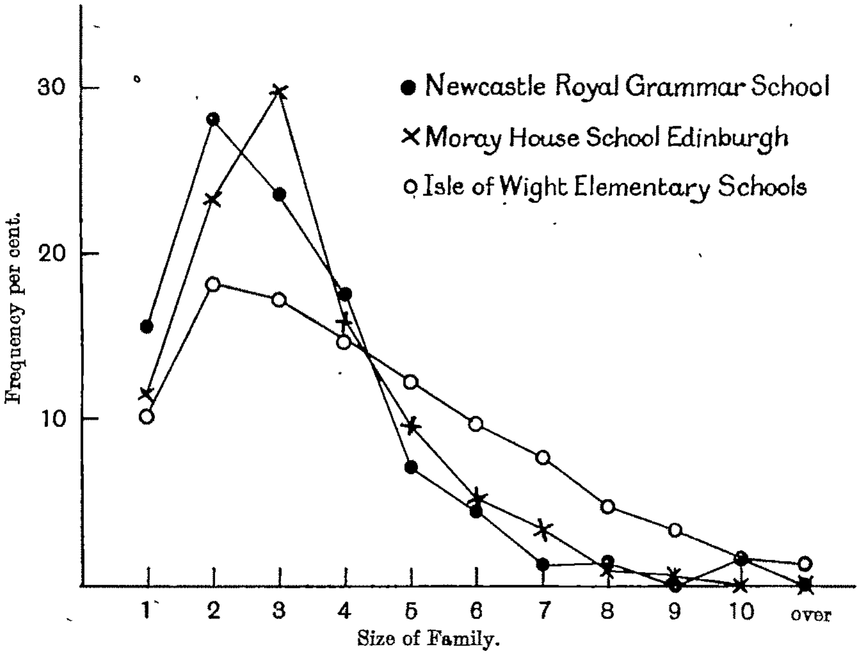

Fig 2.

The same facts shown in the figure may less completely be expressed thus:

	Average intelligence	Average family
1924 unselected children	Not over 100 I.Q.	4·252
395 children in a College Demonstration School	Believed to be high	3·256
386 children in a good Grammar School ...	I.Q. 112 5	3·114

Moreover, the average family of 4·252 of the Isle of Wight is more likely to be an underestimate, due to incomplete families, than is the 3·114 of the Grammar School, where the boys were some years older.

8. CONCLUSION.

We have set out the possible errors in our work as fully as we can. But after allowing for them, we conclude that there is a correlation of approximately $-0{\cdot}2$, between size of family and intelligence of its members, among unselected children. In children selected for intelligence this correlation is masked, but the same phenomenon is shown in another form, *i.e.* by the smaller families to which such children, on the average, belong.

APPENDIX.

Complete data for the 1084 *cases in Isle of Wight*, 1926.

Size of family	Position	Categories of intelligence Low *j*	*i*	*h*	*g*	*f*	*e*	*d*	*c*	*b*	High *a*	Total
1	1st	13	10	8	8	9	13	17	13	9	15	115
2	1st	24	3	10	6	15	14	17	13	14	12	128
	2nd	13	6	8	5	10	8	11	6	7	10	84
3	1st	6	3	4	4	7	4	—	7	13	4	52
	2nd	15	6	5	9	10	10	6	5	8	3	77
	3rd	14	3	5	8	4	5	6	3	3	5	56
4	1st	7	2	3	7	4	1	6	8	1	1	40
	2nd	7	3	7	4	4	2	3	2	2	3	37
	3rd	11	2	5	2	2	4	1	3	3	6	39
	4th	4	3	1	2	7	3	4	2	3	7	36
5	1st	3	—	—	2	2	3	2	2	1	2	17
	2nd	1	3	1	1	1	—	2	—	2	2	13
	3rd	5	4	2	3	7	2	3	—	4	4	34
	4th	16	1	3	5	—	4	4	2	3	1	39
	5th	6	1	1	6	1	3	3	1	—	2	24
6	1st	1	1	2	2	—	—	1	1	1	—	9
	2nd	4	1	1	2	1	2	3	2	2	2	20
	3rd	3	2	3	1	—	3	2	2	—	—	16
	4th	7	2	2	1	—	3	3	1	—	1	20
	5th	6	4	2	2	—	2	1	2	1	1	21
	6th	2	1	4	4	1	2	1	1	1	—	17
7	1st	2	—	—	—	—	1	—	—	—	1	4
	2nd	2	1	2	—	—	—	1	—	2	—	8
	3rd	9	—	4	1	2	1	—	2	—	—	19
	4th	3	2	2	1	—	3	1	1	—	1	14
	5th	5	3	—	—	3	1	—	—	—	—	12
	6th	3	2	1	—	—	—	—	—	2	—	8
	7th	8	4	2	1	1	2	—	4	—	1	23
8	1st	1	1	—	—	—	—	—	—	—	—	2
	2nd	1	—	—	—	—	—	1	—	—	—	2
	3rd	—	1	1	—	—	1	2	—	—	—	5
	4th	3	1	1	1	2	1	—	—	1	—	10
	5th	3	2	1	2	2	—	1	—	—	—	11
	6th	5	1	1	1	—	—	—	—	—	1	9
	7th	1	—	1	2	2	1	—	—	—	—	7
	8th	—	1	—	—	2	—	1	—	—	—	4
9	3rd	1	—	—	—	—	—	—	—	—	—	1
	4th	1	—	—	—	—	1	—	—	—	—	2
	5th	—	1	—	1	—	1	—	1	—	—	4
	6th	1	—	—	1	—	—	—	—	—	2	4
	7th	2	—	—	—	1	—	2	—	1	—	6
	8th	2	1	—	—	1	—	1	—	—	—	5
	9th	1	—	1	—	—	2	1	—	—	—	5
10	5th	1	—	—	2	—	—	—	—	—	—	3
	6th	2	—	1	—	—	—	1	—	—	—	4
	7th	1	—	—	—	—	—	—	—	—	—	1
	9th	2	—	—	1	—	1	—	—	—	—	4
	10th	1	—	2	—	—	—	1	—	—	—	4
11	4th	1	—	—	—	—	—	—	—	—	—	1
	6th	—	—	—	—	1	—	—	—	—	—	1
	9th	—	—	—	—	1	—	—	—	—	—	1
	11th	1	—	—	—	—	1	—	—	—	—	2
12	5th	—	—	—	—	—	—	1	—	—	—	1
13	9th	—	1	—	—	—	—	—	—	—	—	1
14	14th	—	—	—	—	—	1	—	—	—	—	1
15	12th	1	—	—	—	—	—	—	—	—	—	1

THE CONSTANCY OF 'g,' GENERAL INTELLIGENCE[1]

By C. S. SLOCOMBE.

(From the Psychological Laboratory, University College, University of London.)

INTRODUCTORY.

PART I. Investigation to determine whether there is any variation in 'g' as measured by **one single form** *of test repeated with varying material of uniform difficulty.*

- *Investigation described* (p. 94).
- *Results of investigation* (p. 95).
- *Discussion of results* (pp. 95–98).
- *Mathematical criterion* (pp. 98–100).
- *Application of criterion* (pp. 100, 101).
- *Conclusion* (p. 101).

PART II. Investigation to determine whether there is any variation in 'g' as measured by **several independent forms** *of test with varying material of uniform difficulty.*

- *Scheme of testing* (pp. 102, 103).
- *Description of tests* (pp. 103, 104).
- *Treatment of results* (pp. 104–106).
- *Mathematical criteria* (pp. 106–108).
- *Results* (pp. 108, 109).
- *Interpretation of results* (pp. 109, 110).
- *Conclusion* (p. 110).

SUMMARY.

INTRODUCTORY.

THE repetition of Intelligence Tests has been the subject of much investigation of recent years. The researches have sought to determine (*a*) whether tests are reliable, *i.e.* whether results on retesting have by their correlation with the results on initial testing shown that the test is not unduly influenced by chance factors, and (*b*) whether the intelligence of individuals tested remains constant or develops at some definite rate.

The investigation here described is of the second type, and is a further attempt to determine by an improved technique whether intelligence does remain constant.

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[1] Part of thesis presented for the Degree of Doctor of Philosophy in the University of London.

According to Professor Spearman's Theory of Two Factors, "each ability of a person is capable of resolution into two factors: (*a*) a general or common one, entering into all the abilities of the person, termed 'g'; and (*b*) a factor specified to the particular ability concerned, termed 's'." An intelligence test then may be regarded as a measure of General Intelligence and of a number of specific abilities. If the test is properly designed these specific abilities will on the whole cancel one another out, so that the test may properly be called one of General Intelligence. It is in this sense that the equivocal term 'Intelligence' will be used, and the main method of the investigation will be to separate these two factors, by the mutual elimination of the variable specific factors, so that the general one 'g' may be exposed as constant or varying.

It is therefore necessary to determine on repetition of tests:

(*a*) whether there is any significant variation apparent;
(*b*) if so, whether it is due to real changes in 'g'; or
(*c*) whether it is due to some defect in the test used;
(*d*) whether it is possible to obtain a test which is immune from all influences causing variation.

With reference to (*b*) it may be useful to explain what is meant by constancy of 'g'—mental energy—or its physiological correlate. If any individual be tested any number of times by a perfect test, and he be found to obtain always the same relative score (in the proper comparable units) then his Intelligence and mental energy would be held constant. Or if a boy on testing with a perfect test at the age of eight be found placed 100th in the whole population of that age, and 'g' remains constant, then if he were again tested with a perfect test at the age of eighty, he would still be 100th in a population of octogenarians.

PART I.

Investigation to determine whether there is any Variation in 'g' as measured by **One Single Form** *of Test repeated with Varying Material of Uniform Difficulty.*

Investigation described. Four different forms of test were used, Analogies, Reconstruction of Pictures, Cancellation of Groups of Seven Dots, and Dot Patterns.

The subjects tested were 75 boys attending London County Council Schools. The group was homogeneous in respect of age, sex, social and physical environment. Eight repetitions of each form were given, the material being different in each performance, but as far as possible of uniform difficulty. The interval between each test was one week.

Results of investigation. In order to determine whether any new influences are introduced by the continued repetition of one form of test, each of the forms was treated separately. Thus in each form the intercorrelations were calculated of the eight performances. Table I shows the coefficients obtained in the case of Analogies. Other tables of a similar type are not shown.

TABLE I. *Intercorrelations of eight performances of analogies. Interval between performances* = 1 *week.* ($N = 75$.)

	P 1	P 2	P 3	P 4	P 5	P 6	P 7	P 8
P 1	—	86	81	71	54	52	60	65
P 2	86	—	85	75	67	64	73	80
P 3	81	85	—	80	70	74	81	80
P 4	71	75	80	—	68	83	70	79
P 5	54	67	70	68	—	72	73	62
P 6	52	64	74	83	72	—	70	73
P 7	60	73	81	70	73	70	—	84
P 8	65	80	80	79	62	73	84	—

P 1, P 2, ... refer to first performance, second performance, etc.

The following features are to be noted in this table; the coefficients are high (average ·72, P.E. ± ·038), so that the test form has a high reliability, in the narrow sense of the term; there are sporadic variations in the correlations, due doubtless to accidental similarities in the material or other test conditions; and apart from such accidental variations there is a more or less regular lowering of the coefficients in the directions shown by the arrows, and a ridge of high figures along the diagonal. This may be shown clearly by averaging correlations of all performances at intervals of one week ($r_{p_1p_2}$, $r_{p_2p_3}$, etc.), two weeks ($r_{p_1p_3}$, $r_{p_2p_4}$, etc.) up to seven weeks, when a general lowering appears as the interval becomes larger. This is well illustrated in Fig. 1, which shows also similar results obtained from the other three tests.

Discussion of results. How are these results to be interpreted? From Table I it appears that the correlation between neighbouring performances is comparatively high. This would seem to mean that from week to week the ability of an individual (relative to that of others in the group) varies but little, but that with increasing time intervals, his ability changes considerably, relative to similar changes in others. To what is this change due? It would appear to be due to the effect of practice.

It may be interesting to discuss this question of the effect of practice fully—at least in the case of one test form, Analogies. Such discussion will also serve to throw more light on the significance of the terms, practice, and variation or constancy of 'g.' Though Analogies has been taken for purposes of illustration, other forms would probably also yield similar, if perhaps less striking results.

The following is an example of the Analogies test question: "INSTRUCTIONS. In each question a fourth word is wanted which goes with the third word in the same way as the second word goes with the first. Look among the four words on the right (in CAPITALS) for the word that is wanted, and draw a line under it:

Handle : Hammer : : Knob : —. KEY, ROOM, SHUT, DOOR."

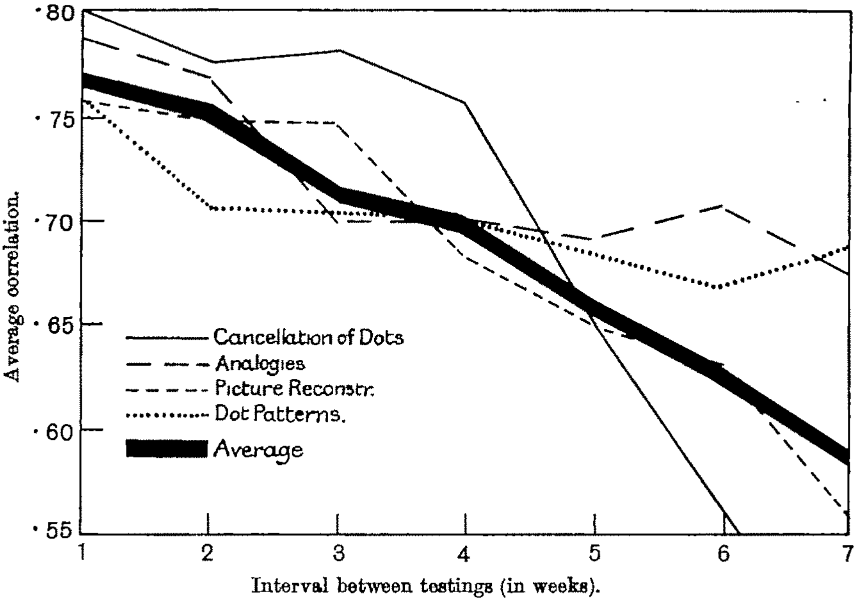

Fig. 1. Showing that the correlation between scores in successive performances varies inversely as the interval. (Same test form.) (Cf. Fig. 3.)

The introspective analyses of persons (including University graduates) doing such a test for the first time revealed the fact that in the majority of cases the following procedure is adopted (in spite of any instructions clearly given). The subject looks at the first of the words in capitals—KEY—compares it with the word KNOB, finds a relation, then looks at the two words on the extreme left to see if the relation fits them; if it does, well and good. If not he then proceeds to ROOM, finds a relation between it and Knob, and sees if that relation holds as between Handle and Hammer, and so on until the correct word is found so that the relations are similar, or fit. This process may be shortened, as in the following example:

Sky : Blue : : Grass : —. TAME, GREEN, WARM, BIG,

where GREEN can be immediately selected, since it is a word commonly associated with grass. In this case the test is not one of ability to find analogies, but of reproduction by association, and is obviously defective.

A more logical method of proceeding with an analogies test, however, would be to study the first two words (as instructed) and find the relation between them—thus Handle 'is the holding part of' Hammer. Then keeping in mind Knob 'is the holding part of' look among the selection of words given for the one which completes the sentence—DOOR.

With practice in the doing of this form of test, the latter quicker and more logical method of procedure comes to be adopted, sometimes slowly and sometimes quickly, according to the subject. Occasionally, of course, the second method is adopted at once.

Suppose two subjects, *A* and *B*, to be presented with this form of test for the first time. *A* commences with the first method, but finding the process of looking backwards and forwards, and trying to fit relations, awkward and irksome, does not score well. On continued repetition of the test, he may gradually—not having time under the speed conditions to think the matter out, and adopt it at once—come to the use of the second quicker method, and so score well in the later performances. Meanwhile *B* has found the first method quite a satisfactory one, being assisted in jumping to the right answer by associative links owing to the badly selected material of which the test is constructed, and scores well from the start. Finding the method satisfactory he continues to use it or changes very slowly. On the whole then, *A* starts poorly and improves quickly; *B* starts well and continues without much improvement, being eventually passed by *A*. (Note—a negative correlation between initial score and relative gain will appear.)

The correlation coefficients will show the influence of these changes in method, for there will be a gradual lowering of the correlation between early and later performances as the interval lengthens. Now suppose there is given an infinity of tests which are worked out in a method similar to that used in the early ones, then the 'g' of *A* and *B* and other subjects could be calculated from them. If also an infinity of tests worked out in the same way as the later ones be given, the 'g' of the subjects could again be calculated. But the correlation between these two measurements of 'g' will most certainly fall short of unity, that is the 'g' measured by the continued repetition of one form of test will have apparently changed. The changes in method adopted by the two subjects above discussed are taken as typical and hypothetical. That some such change in relative position does take place among subjects may be seen by plotting the practice curves of a number of subjects in any series of repeated test performances.

It has been shown by Fig. 1 that in the case of the four forms considered some change is introduced by continued testing, in consequence of which the correlation is lowered. But our exposition has not shown that the lowering of the correlation is significant; it may be due to possible sampling or accidental errors. It must then be regarded as offering rather an indication of possible changes in 'g,' and an explanation, than a definite proof of the existence of variation. It is therefore necessary to apply a strict mathematical criterion to ascertain (*a*) whether the variation is significant, and (*b*) whether the apparent variation is due to real changes in 'g' or to some other cause.

Mathematical Criterion. Professor Spearman's Theory of Two Factors[1] was enunciated and put forward twenty years ago, and therefore requires no detailed explanation here. "According to this theory, each ability concerned is capable of resolution into two factors, the one (designated 's') being specific to this ability alone, whereas the other ('g') is common to all abilities." For example, say the Army Alpha test comprises eight forms of test, Synonyms, Arithmetical Problems, etc. A subject's score in one form of this test (Synonyms) may then be resolvable into two factors, (*a*) a common or general factor—his general ability to do any form of test or other mental process—his intelligence, and (*b*) his ability (quite apart from (*a*)) to do this particular form of test—a specific ability, which by hypothesis will not assist him in the slightest degree in solving Arithmetical Problems.

The theory is of course capable of application to a much narrower sphere, and will be so applied here. For example, in the test form so far mainly considered, Analogies, each subject's ability is capable of resolution into two factors, (*a*) his ability to do Analogies with any material, and under any circumstances, and (*b*) a factor specific to each performance, depending upon accidental peculiarities in the material, printed page, mood of the moment, etc.

But in order that "each ability concerned may be shown to be resolvable into two factors 'g' and 's' *only*, a criterion of conformity is applicable." Professor Spearman has supplied this criterion, together with the means for determining permissible deviations due to errors of sampling. "The said criterion[1] consists in the relation

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[1] "General Intelligence, objectively determined and measured," *Amer. J. Psych.* 1904, xv.

$$\frac{r_{13}}{r_{14}}=\frac{r_{23}}{r_{24}},$$

where the suffixes indicate any of the abilities at issue. This relation has been proved to hold, when and only when, the abilities are resolvable into the said two factors." This may be written in the convenient form,

$$r_{13}r_{24}-r_{23}r_{14}=0.$$

The general formula is applied to a table of correlations of which Table I is an example. It will be seen that such application consists in taking coefficients at the corners of rectangles wherever such are capable of construction in any part of the table. Then the coefficients at the extremes of the diagonals are multiplied, giving two products which should equate, within the limits of five times the probable error. For example in Table I,

$$r_{p_1p_3}\cdot r_{p_2p_4}-r_{p_2p_3}\cdot r_{p_1p_4}=(\cdot 81\times\cdot 75)-(\cdot 85\times\cdot 71)=\cdot 002.$$

This difference between the products from four such coefficients has been termed the Tetrad Difference.

To ascertain whether the two factors only are present it is strictly necessary to apply this equational formula to all possible tetrads in the table, and to obtain the distribution of the differences. If the distribution is normal[2], conformity to the theory of two factors is indicated and proved. But any significant departures from normality will prove the presence of factors other than 'g' and 's' and the possible absence of 'g.'

If there is conformity then it is possible to split up any individual's score in performances into two factors thus:

$$\begin{aligned}\text{Score in first performance} &= g+(s_1^{\alpha}+s_1^{\beta}+s_1^{\gamma}+\ldots).\\ \text{Score in second performance} &= g+(s_2^{\alpha}+s_2^{\beta}+s_2^{\gamma}+\ldots).\\ \text{Score in third performance} &= g+(s_3^{\alpha}+s_3^{\beta}+s_3^{\gamma}+\ldots).\end{aligned}$$

By hypothesis each 's' is uncorrelated with (*a*) the general factor 'g' or (*b*) with any 's' in any performance; that is, the only cause of correlation between any two performances is the common element 'g.'

But suppose s_1^{α} to be identical with s_2^{β},
and s_2^{α} to be identical with s_3^{γ},
and s_1^{β} to be identical with s_3^{β},

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[1] This *Journal*, 1924, xv, 19.

[2] 'Normal' is here used in the special sense of indicating that distribution of values which should be expected from the sole influence of the errors of sampling.

then the correlation between any two performances would be due not only to the 'g' common to them, but also in some measure to the 's' common to each two performances only. Any such element common to a number or group of performances or tests, but not common to all, is termed a Group Factor. It is to be noted that a group factor is not some extraneous factor added to 'g' and 's' but is a structural condition obtaining within.

It will be seen that if a group factor is, or group factors are, present then the correlation between two performances or tests is due to two causes, and it is impossible to ascertain which if any has the predominant influence. In fact it might be that 'g' is altogether absent, and the correlation due solely to group factors.

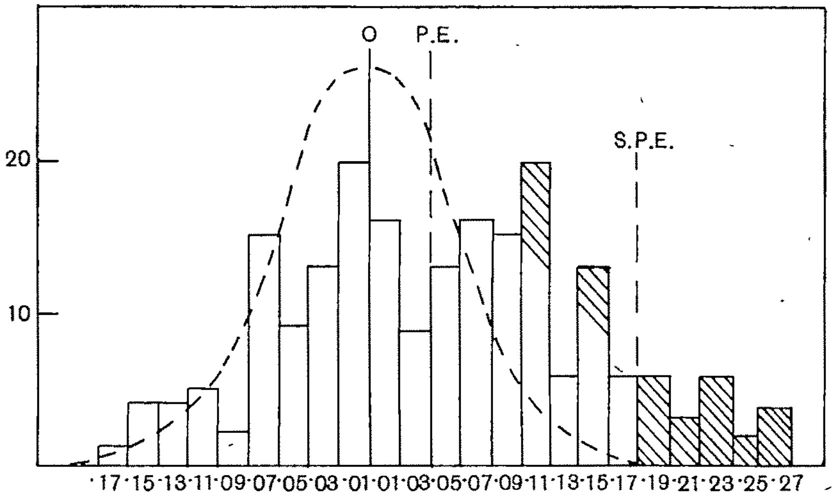

Fig. 2.

But if the conformity criterion given on p. 99 above be applied, and the tetrad differences obtained be found to have a normal distribution, then it is proved that the correlation between performances or tests is due solely to the common 'g,' the specific factors being uncorrelated. If the tetrad differences be not normally distributed then there must be present group factors, and there may be present 'g.' The presence of 'g' can be proved only by showing the correlation of each of the tests with others in which there are no group factors.

Application of criterion. All the possible tetrad differences, 210 in number, have been calculated from Table I. Their distribution is shown in Fig. 2 together with the normal curve. It is to be observed that a considerable area to the right is beyond 5 P.E., and therefore fails to conform. The peaked frequency at 3 P.E. (·11) is also in excess of 3σ of the expected (theoretical) frequency at this interval, as calculated by Yule's formula[1]:

$$\sigma f = \sqrt{\frac{f(N-f)}{N}},$$

f being the theoretical frequency at an interval.

Similar distributions were obtained by a similar treatment of tables of intercorrelations of the other three tests—showing the presence of group factors.

Conclusion. Thus the application of the criterion shows that group factors are present. But their nature can be ascertained only by reference to individual tetrad differences. An examination of the data reveals the fact that the large differences are due to high intercorrelation between early performances, and high intercorrelation between late performances, accompanied by comparatively low correlation between early and late performances. It may be assumed then that there is a group factor common to early performances (*i.e.* something other than 'g' which causes the general intercorrelation) and a different group factor common to the late performances. Thus the early stages are alike, the subjects being in a certain stage of practised ability, the late performances are alike, the subjects also being in a stage of practised ability, but the two stages are different.

Thus in answer to question (*a*), p. 94 above, it may be said that there is an apparent variation in 'g' as indicated by Fig. 1; and that this variation is significantly large, *i.e.* is greater than might happen by chance. In answer to question (*b*) it is mathematically proved that there are group factors present (*i.e.* one common to early performances, and another common to late performances); the presence of these group factors operates to produce an apparent variation in 'g.' It is however impossible to say whether the group factors are the sole cause of the variation, or whether there is not also a real variation in 'g.'

In order then to determine whether there is a real variation it is necessary to consider an investigation specifically designed to eliminate group factors, which by their variation may mask any real variation. The consideration of this investigation constitutes Part II.

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[1] U. Yule, *Introduction to Theory of Statistics*, 1911, p. 305.

PART II.

Investigation to determine whether there is any Variation in 'g' as measured by ***Several Independent Forms*** *of Test with Varying Material of Uniform Difficulty.*

Scheme of Testing. It has been shown that when Intelligence is measured by successive repetitions of the *same* form of test a variation in 'g' is apparent; but owing to the influence of changing group factors it was not possible to ascertain whether there was any real change in 'g.' It was therefore thought that group factors might be eliminated by considering the correlations between *different* forms of test, when the forms are separated by varying intervals.

Accordingly, a scheme of testing was devised in which eight different test forms were used. These forms were:

(A) Analogies.
(B) Opposites.
(C) Classification.
(D) Directions.
(E) Effect Relations.
(F) Synonyms.
(G) Narrative Completion.
(H) Reasons.

These eight forms, together constituting a test, were applied to eight groups of children with different material for each of the two occasions of testing of each group. Each group was tested twice, the interval between testing varying with the group, as shown below.

Groups 1 and 2 retested after interval of 3 months,
4 and 8 ,, ,, ,, 1 month,
3 and 7 ,, ,, ,, 1 week,
5 and 6 ,, ,, ,, 1 day.

The purpose required that the same pair of test forms should be placed (*a*) at different intervals, and (*b*) in different positions in the series. Then comparison of the correlations at these different intervals and positions was expected to show the existence of any variation in 'g.' Thus, suppose r_{AB} is found when the test forms occupy the first and second positions in a test, and when they occupy the seventh and eighth positions, and also found when they occupy the first and eighth positions. Then if 'g' varies it would be expected that the last coefficient would be lower than the other two.

But it is obviously impossible to arrange the forms in these various ways in applying them to one group of children, without introducing other factors. It was therefore necessary to test eight groups, the test forms applied to each group being the same, but varying in arrangement from group to group. A cyclic order of arrangement of the forms was therefore designed. It is shown below.

Sequence	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8
1	A	C	E	G	H	F	B	D
2	B	D	F	H	A	C	E	G
3	C	A	G	E	C	A	G	E
4	D	B	H	F	F	H	D	B
5	G	E	A	C	E	G	C	A
6	H	F	B	D	D	B	H	F
7	E	G	C	A	B	D	F	H
8	F	H	D	B	G	E	A	C

The order of forms was the same in the second test as in the first. The sequence may be said to run from 1 to 8 in the first test and the same in the second. Or if the interval of 1 day, week, month or quarter be disregarded, the sequence of forms in the second test may be said to run from 9 to 16. Thus for example, the pair AB is found in the position 1, 2 in Group 1, in the position 15, 16 in Group 4 and in the position 16, 1 in Group 7. Altogether each possible pair of forms may be correlated when occupying every one of 32 different positions. The orders in the different groups were so arranged that in any set of positions four pairs could be found. This arrangement was made in order that coefficients obtained by the correlation between the members in each of the four pairs could be averaged. This was necessary in view of the fact that the correlation between a pair of forms at one interval obtained from one group of children is to be compared with the correlation between the same pair of forms at another interval, obtained from a different group of children. Such a comparison is hardly legitimate for a single pair, as any differences might be due to variations in the children. But if four pairs in a set of positions are found, then they may be averaged for comparison, as each average compared will be obtained from coefficients from the same groups.

Description of Tests. The test forms used were selected because they have been shown by psychologists to have a high intellective saturation. The material used was in general that contained in tests by Burt, Ballard, Spearman and Tomlinson. The tests were constructed, each containing 16 sheets, 9 being in the Inventive form (in which the children educed the correct answer without assistance) and 7 in the Selective form (in which they selected the correct answer from a number given). Care was taken in the choice of material to obtain equality in the difficulty of the two tests—though in each sheet of each test the material was graded so that the difficulty increased. The instructions for each sheet were set out at the foot of the preceding one, and a definite time (½ min.) allotted for study of each instruction, so that the results should not be influenced by any differences in time devoted to the reading of instructions.

The time allotted to each test sheet was 1¾ min. for the first testing, and 1¼ min. on second testing. Each test was preceded by 25 min. teaching of the procedure for all forms. No fore-practice, in the proper sense of that term, was given. The teaching of the test forms was given in the order in which they would appear in the test.

The groups of children tested were as far as possible selected on the basis of uniformity of race, environment (including type of teacher, and social and physical conditions), age (11–12 yr.), sex, and time of testing. They were boys attending L.C.C. Schools in the East End. No attempt was made to equalize the groups on the basis of ability. Generally it may be said that equality in variability was desired in all groups, rather than equality in absolute ability. Approximately 30 children were contained in each group.

Treatment of Results. The scores for the various forms were intercorrelated by the Product-Moment formula, all forms being taken not only with others in the same testing, but also with all others in the second testing. Similar correlations were calculated for each of the eight groups. A table (II) showing the coefficients obtained in the case of Group 3 is given below. The suffix *a* indicates first testing, *b* the second testing. (Similar tables were prepared for the other seven groups, but they are so unwieldy that they are not presented.)

TABLE II. *Intercorrelation of test forms, Group* 3. ($N = 30.$)

	A_a	B_a	C_a	D_a	E_a	F_a	G_a	H_a	A_b	B_b	C_b	D_b	E_b	F_b	G_b	H_b
A_a	—	74	61	56	45	77	85	41	74	76	55	71	42	64	67	51
B_a	74	—	57	68	67	88	75	58	75	75	71	77	75	63	76	69
C_a	61	57	—	61	52	63	66	51	64	67	64	59	40	55	62	52
D_a	56	68	61	—	69	74	78	58	66	61	72	75	75	76	83	69
E_a	45	67	52	69	—	65	55	68	59	63	60	60	71	51	69	74
F_a	77	88	63	74	65	—	81	60	77	67	82	76	58	78	73	75
G_a	85	75	66	78	55	81	—	59	80	77	76	87	70	76	90	61
H_a	41	58	51	58	68	60	59	—	42	61	60	61	60	51	69	66
A_b	74	75	64	66	59	77	80	42	—	79	73	69	57	75	73	61
B_b	76	75	67	61	63	67	77	61	79	—	75	77	65	69	81	55
C_b	55	71	64	72	60	82	76	60	73	75	—	68	59	82	83	61
D_b	71	77	59	75	60	76	87	61	69	77	68	—	59	93	80	65
E_b	42	75	40	75	71	58	70	60	57	65	59	59	—	71	80	62
F_b	64	63	55	76	51	78	76	51	75	69	82	93	71	—	88	75
G_b	67	76	62	83	69	73	90	69	73	81	83	80	80	88	—	66
H_b	51	69	52	69	74	75	61	66	61	55	61	65	62	75	66	—

From such tables a comparison of correlations between pairs in different positions may be made. But the individual coefficients cannot be compared, and have been averaged in the manner discussed above (p. 103), so that each average includes, or is derived from, coefficients from the same groups as does the average with which it is to be compared. Fig. 3 shows graphically the results of this treatment.

An inspection of these curves reveals the fact that though there are sporadic variations there is no regular variation, *i.e.* on the whole the curves tend to be horizontal straight lines. In fact the average shown by the thick black line is almost a horizontal straight line. This figure should be contrasted with Fig. 1.

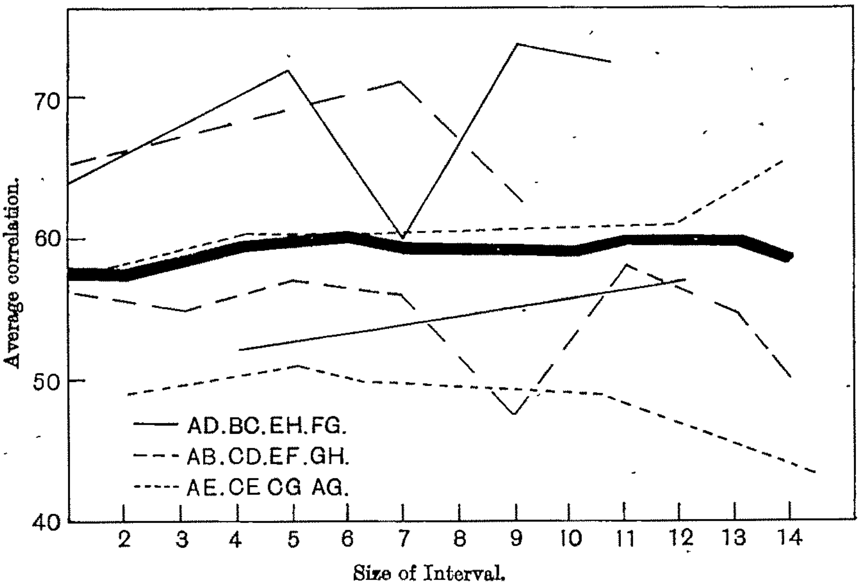

Fig. 3. Showing that the correlation between scores in successive performances does not vary with the interval. (Different test forms.) (Cf. Fig. 1.)

It was shown above (Fig. 1) that when repetitions of one test form are correlated that the correlation varies inversely as the length of interval between repetitions. This shows the presence of group factors, and an apparent change in 'g.' Fig. 3, however, shows that when two different forms are correlated there is no such variation in intercorrelation.

Thus it would seem that the correlation between two different forms of test (*i.e.* the reliability) does not vary with the length of interval. (It may be mentioned that the maximum interval represented in Fig. 1 is 7 weeks, but that the maximum interval represented in Fig. 3 is 12 weeks.) If this conclusion be accepted, it is therefore shown that 'g' as measured by the repetition of different forms of test does not apparently vary from day to day, week to week, month to month, or quarter to quarter. Before discussing the effect of practice with a test so constructed, it will be advisable to consider the application of the mathematical criteria.

Mathematical Criteria. (1) One criterion consists as before in the relation

$$\frac{r_{13}}{r_{14}} = \frac{r_{23}}{r_{24}} \qquad \text{......(1)},$$

or in its equivalent form

$$r_{13} \cdot r_{24} - r_{14} \cdot r_{23} = 0 \qquad \text{......(2)}.$$

It will be remembered that the method of application is to calculate the tetrad differences, as the results of equation (2) are termed, in the case of all possible tetrads in a table of correlations. Such a method is however exceedingly laborious, particularly if applied to a large table such as Table II, which would yield over 5000 differences. Shorter methods must therefore be used.

(*a*) The table may be divided into four quadrants, and the average of the coefficients of each quadrant used in equation (2) above; which then becomes

$$\bar{r}_{aa} \cdot \bar{r}_{bb} - \bar{r}_{ab} \cdot \bar{r}_{ba} = 0 \qquad \text{......(3)},$$

where $\bar{r}_{aa}$ is the average intercorrelation of forms in first test, $\bar{r}_{bb}$ is the average intercorrelation of forms in second test, and $\bar{r}_{ab}$ and $\bar{r}_{ba}$ the average of the correlations of every form in the first test with every form in the second. This results in the case of Table II in the equation

$$\cdot 63 \times \cdot 71 - \cdot 67 \times \cdot 67 = \cdot 003.$$

Unfortunately the formula for calculating the probable error of this tetrad difference has not yet been derived.

(*b*) A second short method will therefore be used. For this purpose the individual test forms are pooled (by the formula for the Correlation of Sums)[1] in sets of four, so that a smaller table results. Table III shows the correlations obtained by such pooling in the case of Group 3. It will be seen that this is really a summarized form of the large table (II). Similar summarized tables not shown for the other seven groups were prepared.

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[1] This *Journal*, 1913, v, 419.

TABLE III. *Intercorrelation of pools of test forms. Group* 3. *Interval between testings*, 1 *week*. (*Cf. Table II.*)

	$(A_a+B_a+C_a+D_a)$	$(E_a+F_a+G_a+H_a)$	$(A_b+B_b+C_b+D_b)$	$(E_b+F_b+G_b+H_b)$
$(A_a+B_a+C_a+D_a)$	—	90	91	84
$(E_a+F_a+G_a+H_a)$	90	—	89	89
$(A_b+B_b+C_b+D_b)$	91	89	—	87
$(E_b+F_b+G_b+H_b)$	84	89	87	—

From these smaller tables the tetrad differences were calculated, the probable error of the differences obtained being calculated by the ordinary formula[1]. The differences so obtained are shown below.

Group 1	$+ \cdot 010 \pm \cdot 023$,	Group 5	$+ \cdot 040 \pm \cdot 035$,
2	$+ \cdot 015 \pm \cdot 026$,	6	$- \cdot 025 \pm \cdot 044$,
3	$+ \cdot 036 \pm \cdot 023$,	7	$- \cdot 033 \pm \cdot 023$,
4	$+ \cdot 045 \pm \cdot 020$,	8	$+ \cdot 060 \pm \cdot 035$.

According to the criterion, if the tetrad differences equate to zero, within the limits of sampling error, then, and only then, are two and only two factors present. As none of the differences shown above is significantly large, it may be taken that 'g' and 's' only are present (*i.e.* group factors are absent).

(2) This result may be expressed in an alternative manner. Equation (3) above, which is a special case of equation (1), may be set out in a different form, thus

$$\frac{(\bar{r}_{ab})^2}{\bar{r}_{aa} \cdot \bar{r}_{bb}} = 1,$$

or

$$\frac{\bar{r}_{ab}}{\sqrt{\bar{r}_{aa} \cdot \bar{r}_{bb}}} = 1 \qquad \text{......(4).}$$

The similarity between this equation (4), and the one given by Professor Spearman, is to be noted[2]. His formula, as a special case of the formula for the Correlation of Sums of similar tests, is

$$r_{(a_1+a_2+\ldots \text{ to infinity})\,(b_1+b_2+\ldots \text{ to infinity})} = \frac{\bar{r}_{ab}}{\sqrt{\bar{r}_{aa} \cdot \bar{r}_{bb}}} \qquad \text{......(5).}$$

Taking these two equations (4) and (5) together it may then be said that if 'g' and 's' only, that is, no group factors are present, the correlation between an infinity of forms similar to those used in the first test, and an infinity of forms similar to those used in the second test will be unity, within the limits of sampling error. But 'pure' or 'absolute' General Intelligence may be defined as that which would be measured by an infinity of independent forms of test. An infinity of independent forms is taken, because in a limited number of forms the specific factors might not (and generally do not) cancel out on pooling or summing. But in an infinity, by hypothesis, they must do so, and leave only 'g.' It may then be said that if

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[1] This *Journal*, 1924, xv, 19.

[2] This *Journal*, 1913, v, 421. (There is a misprint on the page quoted. There should be no root sign in the numerator.)

$$r_{(a_1+a_2+\ldots \text{ to infinity})\,(b_1+b_2+\ldots \text{ to infinity})} = 1,$$

then will the Intelligence measured by the first test be the same as that measured by the second. This is the positive converse of the tetrad differences criterion.

It was observed above that this formula (5) is a special case of the Sums Formula. In point of fact the formula is that used for correcting such a correlation of sums for attenuation due to 'accidental errors[1].' Thus by the general Sums Formula the correlation of the whole first test with the whole second test may be obtained, that is, the raw reliability of the test as a whole. By the attenuation formula (5) this reliability may be corrected.

Applying these formulae to Table III and similar tables for other groups, the following results are obtained.

TABLE IV. *Showing that (a) the reliability of a repeated test is independent of the length of interval, and (b) that 'g' as measured by such repeated test does not vary, up to a period of three months.*

Interval	Group	Raw reliability of whole test	Corrected reliability	Probable error
3 months	1	·94	·99	·015
3 ,,	2	·95	1·01	·013
1 month	4	·95	·99	·013
1 ,,	8	·93	1·04	·020
1 week	3	·93	1·00	·018
1 ,,	7	·95	1·02	·013
1 day	5	·91	·99	·027
1 ,,	6	·90	1·01	·025

Results. Thus it has been shown (p. 105) that in the tests used, no group factors are present. By Table IV it is shown that under such circumstances, 'g' as measured by repetitions of a number of different forms of test *remains constant*, up to a period of at least three months. (Column headed Corrected Reliability gives correlation between 'g' of first test with that of second test.) This means that, if accidental errors be eliminated, then subjects retain their same rank order whenever tested by such tests.

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[1] This *Journal*, 1910, III, 279.

Interpretation of Results. It will now be profitable to discuss this result in relation to the results obtained and discussed in Part I. There, there were eight repetitions of four test forms. For the purpose of that part, these were treated separately—and an apparent variation in 'g,' as measured by repetitions of each test form, was found. If now the scores obtained in the four forms are combined (being first reduced to equal Standard Deviation), for each of the performances, and treated as eight separate tests, it should be possible to see whether any variation (apparent) is found in 'g' as measured in this way. Only the first, second, seventh and eighth testings have been so treated, as yielding a maximum interval, and consequent maximum variation—if any. The intercorrelation of these combined scores is shown in Table V.

TABLE V. *Showing intercorrelation of early and late* COMBINED TESTS-(A + B + C + D) *described in Part II.*

	1st test	2nd test	3rd test	4th test
1st test	—	·788	·731	·723
2nd test	·788	—	·886	·850
3rd test	·731	·886	—	·910
4th test	·723	·850	·910	—

The tetrad differences calculated from this table are ·067 and ·087 (P.E. ± ·029). As these are less than 3 P.E., it may be taken that, within the limits of error, no group factors are present.

Using the alternative form of the formula, as was done in obtaining the data for Table IV, the following result is obtained:

$$\text{Reliability} = r_{(1+2)(7+8)} = \cdot 870.$$
$$\text{Corrected for attenuation} = \cdot 945 \text{ (P.E. } \pm \cdot 024).$$

As this corrected reliability is less than unity by less than 3 P.E., it may be taken that the 'g' as measured by the early tests is nearly the same as that measured by the later tests.

Thus a rather striking conclusion has been reached. For when 'g' is measured by repetitions of one form of test, practice enters as an influence producing an apparent variation (possibly periodic). But when a number of such repeated forms are combined, it manifests no such influence.

How is it then, that practice, though present and producing an increase in score, does not influence 'g' as measured. It is inferred that the influence of practice is specific to the form of test. Thus when a number of test forms are combined practice enters as an uncorrelated specific factor. (This agrees with the generally accepted belief that there is no Transfer of Training, from one mental process to another.)

Conclusion. The results of this part may be summarized by reference to the four questions for determination enumerated on p. 94 above.

It has been shown that (*a*) a variation in 'g' as measured in a certain manner may be apparent; but it is concluded that (*b*) such variation, if present, is not a real one, but is due to defects in the tests used for measurement, the defects consisting in the presence of group factors which vary.

It is shown that (*c*) it is possible to obtain a test (composed of a sufficient number of independent forms) which is immune from influences causing change; and that (*d*) the measurement of Intelligence by such a test shows that 'g' is constant.

SUMMARY.

(1) It was found that when 'g' is measured by repeated testing with one test form, such as Analogies or Picture Reconstruction, that the correlation between performances varies inversely as the length of interval separating them. From this it was concluded that 'g' apparently varies.

(2) The application of mathematical criteria, however, revealed the influence of practice, operating to produce group factors, which were found to vary. It was found impossible therefore to determine by this investigation whether there is any real variation in 'g.'

(3) In a further investigation, involving the repeated testing of 232 children, with a test comprising eight test forms, it was found that the correlation between members of pairs of forms was independent of the length of interval. It was further found that no group factors were present in these tests.

(4) This result proved that 'g' really is constant, and that any apparent variations found by testing are to be attributed to varying group factors involved in the tests used, and that the appearance of such variation is to be regarded as a serious defect in tests.

(*Manuscript received* 26 *April* 1926.)

CADENCE AND PHRASE TESTS IN MUSIC

BY H. LOWERY.

(*From the Psychological Laboratory, University of Leeds.*)

1. *Introduction* (pp. 111, 112).
2. *The Cadence Test* (pp. 112–115).
3. *The Phrase Test* (pp. 115, 116).

1. INTRODUCTION.

IT will be generally admitted that the success of a musical performance depends upon the technical skill of the performer and his ability to give an intelligent interpretation of the composer's meaning. In order then to study the musical ability of an individual, the technical and interpretative sides of musical performance must be distinguished from one another. Whereas in the case of the performer both sides are prominent, with the intelligent listener, the latter only is involved.

Considering first the question of technique, it may be noted that many music teachers, in their recognition of the fundamental importance of technique, stress technical mastery at the expense of intelligent interpretation with the result that many so-called musical performances degenerate into mere technical display. But even perfection in technique cannot compensate for failure to deal adequately with the interpretative side of musical performance, and since a certain measure of technical mastery can be attained without consideration of interpretation, it seems that the only valuable tests of musical ability must be those concerned with ability to understand and interpret music. This view appears to be supported by leading musical critics who, in passing judgment on an artist's performance, assume his possession of the necessary instrumental technique and dwell entirely on his degree of success in revealing the intentions of the composer.

It may further be noted that technical perfection in practical music depends largely upon physical and mental factors such as a good state of health and control over the nervous and muscular systems, being on a par in these respects with dancing and games involving bodily action. Where these factors are lacking, musical ability is still possible and may manifest itself in an appreciation of good music. Thus tests of musical ability must cover primarily the interpretative side of music and may be independent of technical equipment.

One criticism of Seashore's[1] tests of aural perception, considered as tests of musical ability, would be their failure to do justice to those factors coming under musical interpretation; thus the tests on discrimination of differences in pitch, rhythm and time, have but a remote connection with the intelligent rendering of music.

The following cadence and phrase tests were devised during an attempt to frame tests of musical ability covering some of the factors necessary to the appreciation and interpretation of music. They are no doubt capable of considerable development, and in particular an attempt might be made to carry further the cadence tests by using cadences exactly as they appear in the works of the great composers.

2. THE CADENCE TEST.

The following cadences are employed: (1) the full close or perfect cadence (V*a* to I*a*), (2) the plagal cadence (IV*a* to I*a*), (3) the various forms of half-close or imperfect cadence (I*a* to V*a*, II*a* to V*a*, IV*a* to V*a*, VI*a* to V*a*), (4) the false or deceptive cadence (V*a* to VI*a*).

[N.B. In the harmonization, only four-part chords are used founded upon the triad, either the root or fifth of the triad being doubled to complete the four parts. The root of the triad is always in the bass, thus 'V*a*' means a four-part chord with the fifth degree of the scale as root, together with the third and fifth notes above it and either the root or fifth doubled. The '*a*' in 'V*a*' indicates root position of the chord. The chords are used in various positions consistent with the root of the triad being in the bass.]

The test is in two parts, Part I consisting of 25 pairs of cadences in a major key (C major) and Part II of 25 pairs of cadences in the relative minor (A minor).

Before the test is given, the attention of the subjects is drawn to the various ways of ending musical sentences by demonstration on the piano. The subjects are then told that two pairs of chords will be sounded in succession and are asked to state whether the second pair seems more or less complete than the first; if more complete they are to record *M*, if less complete to record *L*, and so on for each example. The key chord is sounded before commencing the test.

The various cadences employed in the test are given at the end of this paper together with the appropriate letter (*M* or *L*) for each example indicating the correct judgment as revealed by the standard classical compositions of the last three centuries; thus, the perfect and plagal cadences are considered as most complete, the various forms of the half-close as next in order of completeness, and the deceptive cadence least complete.

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[1] Seashore, *The Psychology of Musical Talent.*

Preliminary work with this test gave the results shown in Table I. Fifty girls between the ages of 12 and 14 years were tested, the cadences being played on a grand piano. Though the piano is fairly satisfactory for the purpose, the test would undoubtedly be improved if given on an organ owing to the sustaining qualities of this instrument which would bring out the full effect of the cadences.

TABLE I. *Cadence Test.*

Part I		Part II	
Example	Percentage correct	Example	Percentage correct
1	100	1	100
2	90	2	88
3	66	3	74
4	54	4	66
5	96	5	70
6	66	6	30
7	62	7	42
8	50	8	70
9	22	9	36
10	94	10	50
11	22	11	58
12	88	12	32
13	72	13	74
14	50	14	28
15	52	15	38
16	78	16	50
17	34	17	82
18	44	18	70
19	84	19	66
20	42	20	58
21	52	21	80
22	50	22	84
23	80	23	54
24	38	24	56
25	38	25	38

TABLE II. *Phrase Test.*

Example	Percentage correct	Example	Percentage correct
1	100	26	42
2	62	27	30
3	66	28	48
4	74	29	84
5	68	30	46
6	84	31	74
7	52	32	76
8	52	33	68
9	74	34	66
10	58	35	42
11	48	36	50
12	58	37	42
13	78	38	70
14	68	39	82
15	64	40	32
16	68	41	28
17	72	42	58
18	74	43	76
19	60	44	70
20	64	45	70
21	70	46	66
22	84	47	68
23	46	48	36
24	52	49	62
25	62	50	64

Twenty of the girls had received instruction in pianoforte playing, some for as long a period as 4 years, but the results for these girls are not in any way distinguishable from those of the girls who had not received any such instruction, in fact one girl with 4 years' instruction had only 52 per cent. of the examples correct. Unfortunately much of elementary pianoforte tuition is melodic in character, being concerned mainly with 'five-finger exercises' and scales, hence piano pupils have practically no advantage in this test, where harmony is prominent, over those who have had no tuition.

The following is the analysis of the results for the 50 girls tested:

No. having between 80 % and 100 % correct (highest mark 80 %)	1
,, 70 % ,, 79 % ,, ,,	7
,, 60 % ,, 69 % ,, ,,	25
,, 50 % ,, 59 % ,, ,,	13
No. having less than 50 % correct (lowest mark 46 %)	4
	50

Table I shows that in many cases there is good agreement between the judgments of the subjects and the expected judgments: further, on an examination of the examples where marked disagreement occurs, explanations of the disagreement are available.

In Part I, examples 9, 11, 17 and 24, the strong voting of the subject against the correct judgments may be due to the fact that the notes of the chords in the cadences which are 'more complete' in these examples are separated by wide intervals and the soprano notes are generally high up the stave. It is just as though many of the subjects were incapable of appreciating these cadences on account of inability to synthesize the constituent notes into chords. In support of this explanation it may be noticed that in those examples (*e.g.* Part I, 10, 12 and Part II, 17) where the subjects were expected to vote *against* such cadences as those mentioned above, they did so strongly.

Some subjects seem to have experienced difficulty in forming their judgments in the examples in which one of the cadences ended with a chord having the fifth degree of the scale as root (*e.g.* Part I, 14, 20, 25) and even in examples where the other cadence ended on the tonic chord (as in examples 9 and 10 of Part I), which have already been discussed above. It may be noted that the chord built on the fifth degree of the scale as root has a certain satisfying effect due to its being the tonic chord of the key having, as key signature, the key signature of the test with one sharp added. The fact that in each of these examples the subjects are not strongly in agreement with the expected judgments is not against the test but against the subjects themselves for it points to a deficiency of the musical memory necessary to keep the original tonality of the tests in mind.

That some subjects followed the melody notes only and did not attempt to judge of the general effect of the chords seems likely from the results of Part I, examples 4, 8, 11, 15, 18, 21, 22, though examples 11 and 21 are complicated by the spacing of the notes in the chords as noted above. In these examples many subjects voted *for* the cadence whose second chord has the tonic as soprano note though from a consideration of the cadence as a whole they would be expected to vote against it.

The most outstanding point in connection with the judgments of Part II is the tendency to favour cadences ending on V*a* even when the partner cadences end on I*a*. This is noticeable in varying degree in examples 6, 11, 12, 16, 19, 20, 23, 25. There is also a tendency to favour cadences ending on VI*a* as in examples 7, 10, 14, 15, 24. It may be noted that both the chords V*a* and VI*a* of the minor key are *major* chords, *i.e.* they contain a major third, and they seem to be so attractive to subjects of small musical ability that the judgments of these subjects are biassed in their favour. (The great popularity of the interval of the major third has been pointed out by Valentine, this *Journal*, 1914, VII, p. 118; also *Experimental Psychology of Beauty*, Revised ed. p. 103.)

In view of the above explanations, the preliminary results obtained may be regarded as favourable to the test and the examples in which the subjects voted strongly against the accepted judgments are of value as affording scope for those subjects of marked musical ability.

3. THE PHRASE TEST.

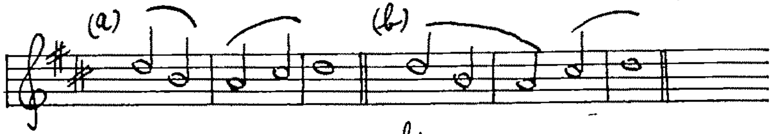
For success in musical study, whether it be as performer or merely as an intelligent listener, too great attention cannot be paid to the appreciation of musical phrases, that is, the grouping of notes in a composition to give meaning to the whole. One has not far to go to find performers, for example, who astonish by their mere technical display but who fail utterly to bring satisfaction or meaning into their performance owing to an apparent lack of ability to group the notes of the composition in an intelligent manner—witness the reports of adjudicators at the numerous musical festivals.

Evidently the first step on the way towards a recognition of the meaning of any particular grouping of notes must be the ability to distinguish differences between the various ways of grouping a given series of notes, and it is this ability which the present phrase test aims at testing.

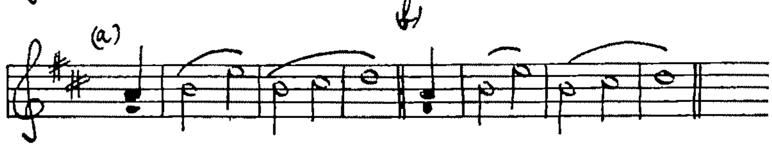
The test consists of 50 pairs of musical sentences, the first 30 containing 5 notes in each sentence of the pairs, the next 10 containing 6 notes and the remaining 10, 7 notes. The *notes* in the two sentences forming a pair are the same, the grouping of the notes in the second sentence being the same or different from that in the first sentence.

After a brief demonstration on the piano of the phrasing of notes the subjects are asked to judge whether the notes in the second sentence of each pair are phrased in the *same* way or *different* from those in the first sentence and to record *S* or *D* accordingly. The following specimens indicate the nature of the examples forming the test:

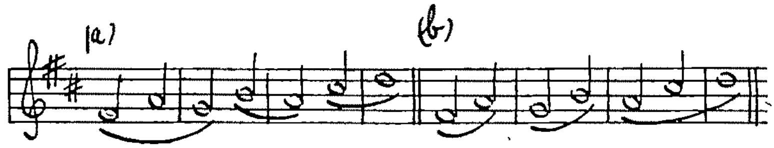
Ex. 1.



Ex. 2.



Ex. 3.



The results for the 50 girls who were tested are shown in Table II above. The analysis of these results is as follows:

No. having between	80 % and 100 %	correct (highest mark 82 %)		2
,,	70 % and 79 %	,,	,,	11
,,	60 % and 69 %	,,	,,	24
,,	50 % and 59 %	,,	,,	12
No. having less than 50 % correct				1
				50

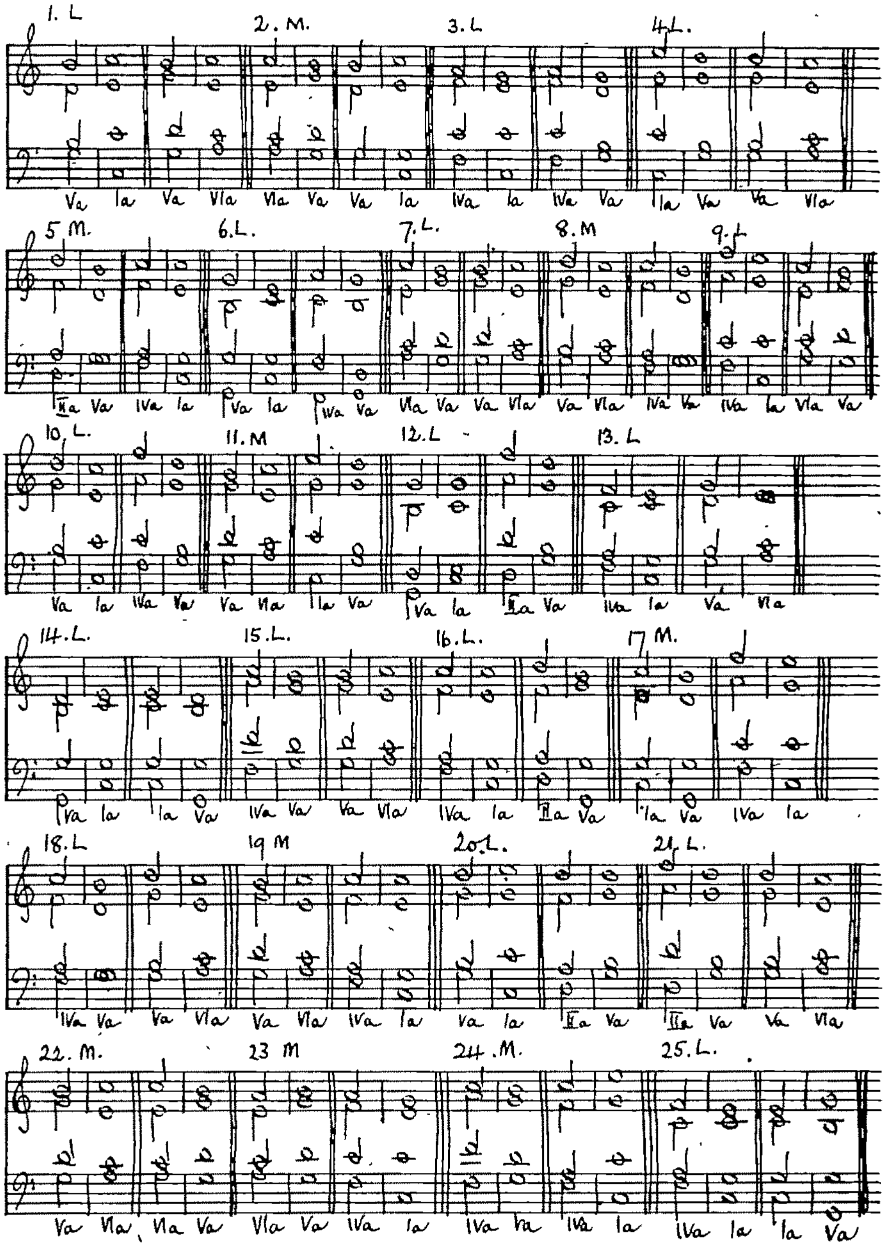
It will be seen that the above tests have the advantage that they may be given to a large body of subjects simultaneously and they are based upon conditions actually occurring in practical music.

It is hoped to develop further the above tests along with others in order to cover as completely as possible the factors involved in the appreciation and interpretation of music.

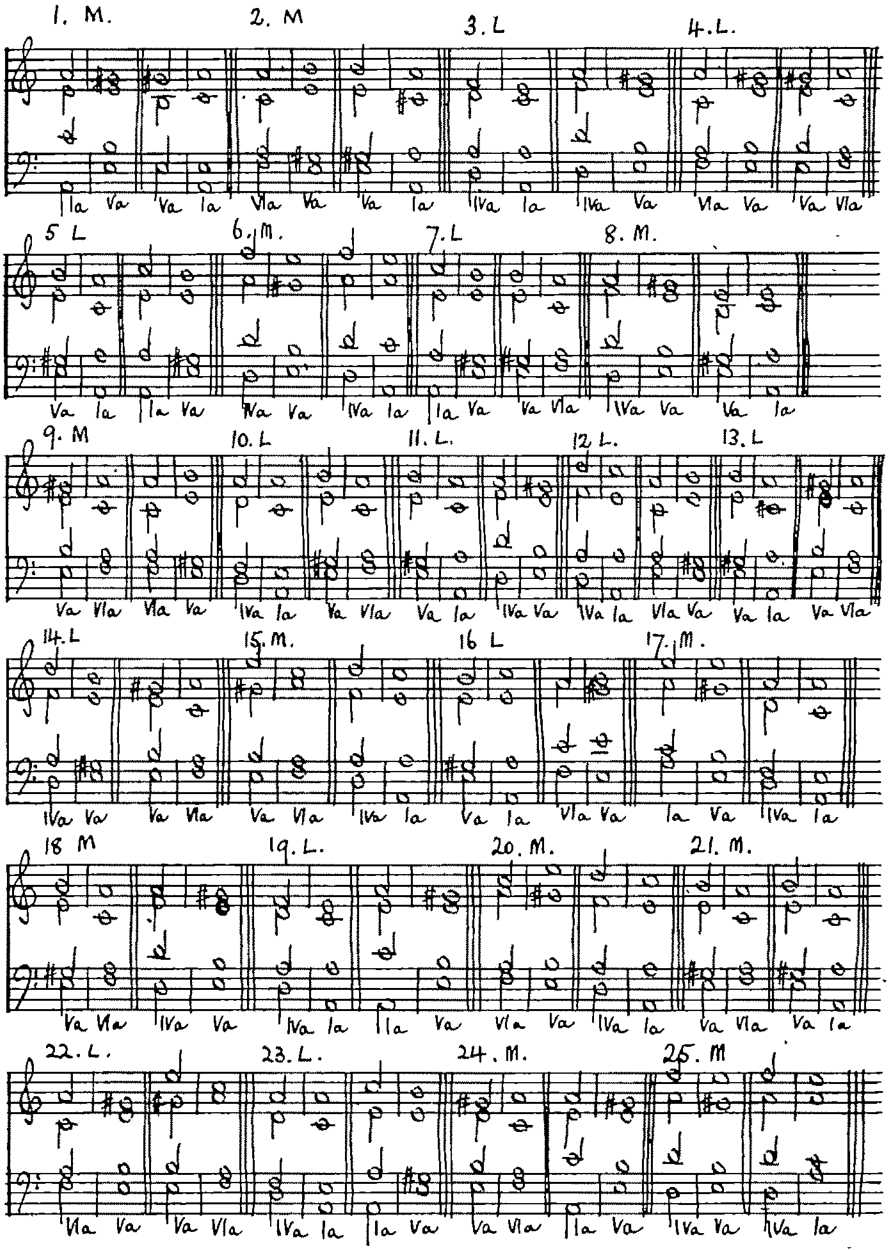
I desire to express my hearty thanks to Dr Ll. Wynn Jones, of the Psychological Laboratory, University of Leeds, for his helpful advice in connection with the work.

EXAMPLES EMPLOYED IN CADENCE TEST.

Part I. Major Key.


1. L
2. M.
3. L
4 L.
Va Ia Va VIa VIa Va Va Ia IVa Ia IVa Va IIa Va Va VIa
5 M.
6. L.
7. L.
8. M
9. L
IIa Va IVa Ia IVa Ia IVa Va VIa Va Va VIa Va VIa IVa Va IVa Ia VIa Va
10. L.
11. M
12. L
13. L
Va Ia IVa Va Va VIa Ia Va IVa Ia IIa Va IVa Ia Va VIa
14. L.
15. L.
16. L.
17 M.
IVa Ia Ia Va IVa Va Va VIa IVa Ia IIa Va Ia Va IVa Ia
18. L
19 M
20. L.
21. L.
IVa Va Va VIa Va VIa IVa Ia Va Ia IIa Va IIa Va Va VIa
22. M.
23 M
24. M.
25. L.
Va VIa VIa Va VIa Va IVa Ia IVa Va IVa Ia IVa Ia Ia Va

Part II. Minor Key.



(*Manuscript received* 23 *June* 1926.)

OBSERVATIONS ON THE UNRELIABILITY OF SUBJECTIVE REPORTS OF EMOTIONAL REACTIONS

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THE following observations were made on the group of normal individuals (64 medical students) who underwent the psychogalvanic experiments reported in a preceding paper[1]. We there have described the stimuli used and the experimental procedure (no external current, string galvanometer, records on rolling film paper). Though these experiments were not undertaken primarily in order to find out those topics which were of especial importance for the individuals under test, we examined the records for this also. The students were asked after the test to give a subjective account of their experience during the experiment. The word list was presented to each of them and the request was made to mark all those words which had elicited, during the experiment, a definite emotional reaction. The results of the experiments also were discussed with many of the subjects after the records were developed and the reactions identified.

The question may arise whether a galvanic response always corresponds to what we call an emotion. There are findings in patients which suggest that the galvanic skin reflex—in some pathological cases at least—may function independently of higher functions of the personality, though from all the work which has been done on the psychogalvanic phenomenon it is evident that, as a rule, a deflection of the string is based on an affective swing. Our experiments corroborate this.

The list of 50 stimulus words which contained terms indicative of a variety of actual life situations was read twice during the experiment (with a few minutes' interval); the subjects were asked to make no response. In the accompanying table we give the stimulus words arranged according to the frequency of the reactions during the first reading. Following each word is indicated the number of individuals (as percentage of the group of 50 students here considered) who gave

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[1] This *Journal*, XVII, pp. 54–69.

TABLE.

(*Reactions and reports as percentages of possible maximum.*)

	Reactions		Reports
Stimulus words	1st read.	2nd read.	1st read.
"Name" (1st)	84	68	36
"Name" (2nd)	84	60	24
naked body	80	42	12
"Name" (3rd)	74	40	28
kiss	72	40	34
husband—wife	66	36	14
intercourse	66	34	8
self-abuse	62	42	20
Jesus Christ	62	32	34
the dean's office	56	52	20
prostitute	52	40	2
disappointment in love	52	26	6
misspent youth	48	20	4
fraternity	46	34	10
lover	46	30	26
passionate	46	24	6
kicked out	46	22	20
success	44	20	24
insane	44	20	6
bonehead	42	14	8
stubborn	40	20	2
stingy	40	16	8
suicide	38	26	6
unchaste	38	18	4
poor health	38	16	4
snob	36	34	10
unpaid bills	36	24	2
brother	36	18	16
anatomy	36	18	4
clumsy	36	18	4
Jewish	36	14	8
Catholic	36	4	0
sister	34	20	14
sneak	32	14	10
cheating	28	22	10
inferior	28	14	6
unpopular	28	12	4
lazy	26	24	4
weakling	26	20	8
egotism	26	18	6
immoral	26	14	2
mother	26	12	30
frigid—impotent	24	28	6
ambitious	24	14	26
hot-headed	22	32	2
father	22	16	28
undersized	20	20	4
childish	20	8	2
stupid	18	24	0
lonely	16	18	16

galvanic reactions to the first and second readings of each word and who reported positive emotional responses to the first reading.

No word was reacted to by all subjects but all words elicited responses

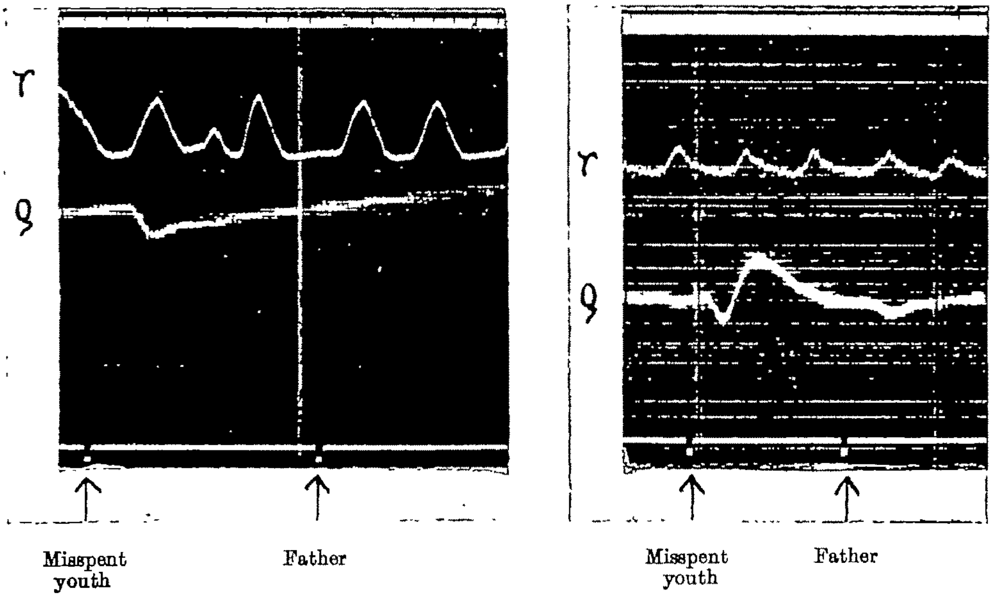

Fig. 1. Corresponding parts of the records (*g*, galvanic and *r*, respiratory curves; stimuli marked on base line) of two individuals whose marked reactions to 'misspent youth' were not reported, whereas there was a false account of emotional response to the word 'father.'

from some individuals. The number of responses varied greatly with the different words, the range being from 16 per cent. to 84 per cent. of the possible maximum. Reactions were most frequently obtained from the presentation of the individual's name. Next in frequency were the responses to sex topics and words associated with the school interests of the subjects. Reactions to the second reading were as a rule less numerous, the frequency averaging from two- to three-fifths that of the first reading. To a few words which gave only a low number of reactions there was a slight increase in response at the second reading.

The reports given by the subjects after the experiment showed marked discrepancies with the actual galvanic reactions (Table). It became evident that none of the individuals tested was able to give an exact account of his emotional reactions; in everyone there was a tendency to overlook or to over-emphasize the response to specific contents. None of the subjects reported more than 37 per cent. of his galvanic reactions; most of them gave account of only 10 to 25 per cent. On the other hand, there were reports of emotional responses with many words where the galvanic curve did not indicate any deflection. Almost every individual reported the memory of a few affect reactions for which there was no substantiation in the form of a galvanic wave; in a few subjects these false reports even exceeded the correct ones. That the responses overlooked were not merely those which caused a slight deflection of the string may be seen in Fig. 1, which represents corresponding parts of records from two individuals. Although the stimulus 'misspent youth' was followed by marked reactions in both subjects, no account of this affect response was given in either case. On the other hand, definite emotional reactions were reported to the word 'father' though there had been no response in one case and only a very slight response in the other.

It was found that the relation of reactions given to reactions reported varied greatly according to the topic, and also that the number of false reports differed markedly with different words. In order to illustrate the principal points a few words of our list with their reactions and reports are given in a diagrammatic way (Fig. 2). These data were obtained from 50 students for whom the stimuli had been exactly identical (except of course for the name). The blank column represents the galvanic reactions, the dark column the reports. The individuals who gave reactions or reports with a specific stimulus word are calculated as a percentage of the 50 individuals who were exposed to the same stimulus word. This percentage is indicated by the height of the columns. The black part of the second columns represents in percentage the frequency of the correct

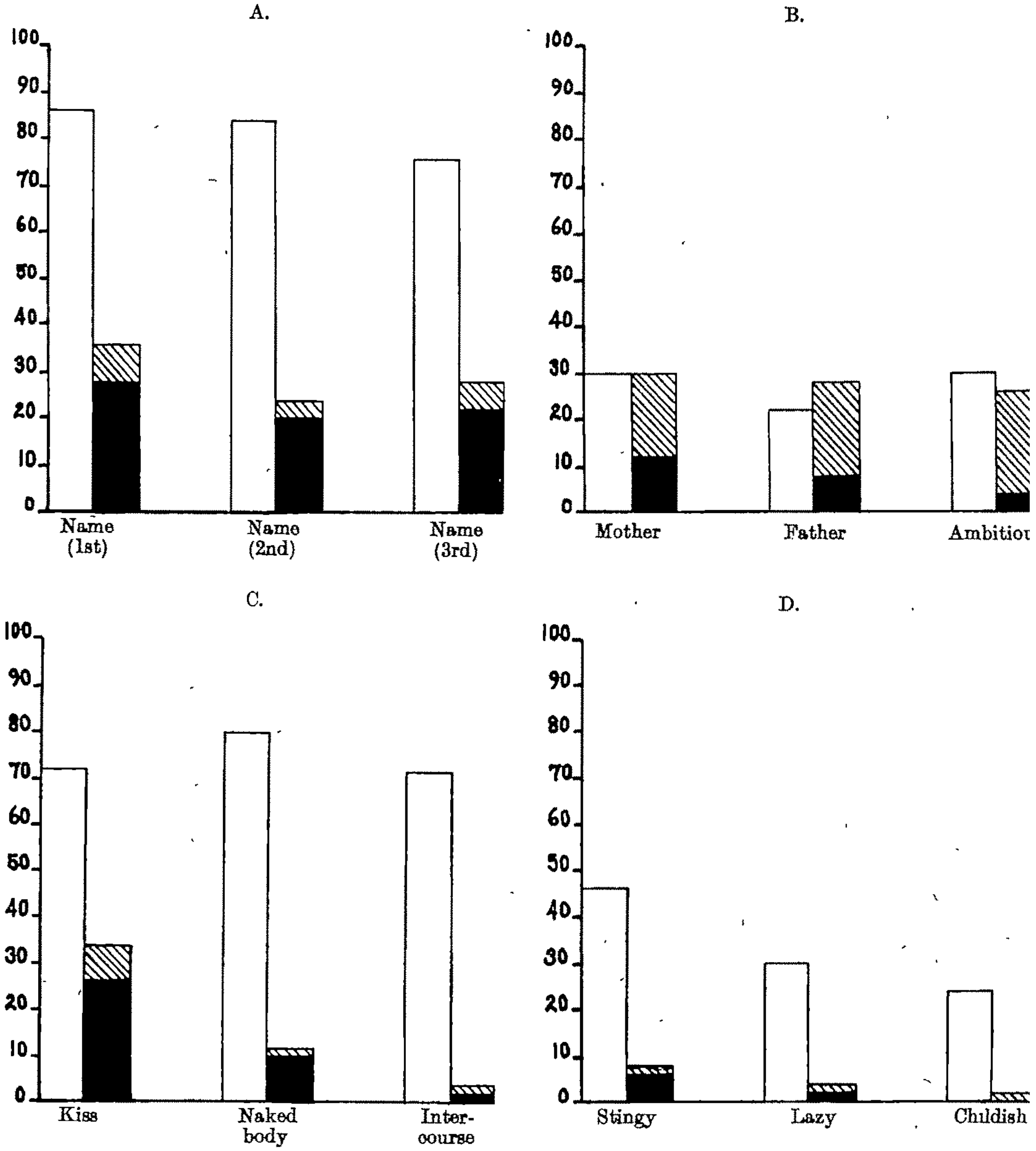

Fig. 2. Diagram showing the frequency of reactions and of subjective reports of reactions to a few of the stimulus words. The percentage as calculated for 50 individuals is indicated by the height of the columns. The blank column gives the percentage of actual reactions; the shaded column gives the percentage of the reports, the black part representing the correct reports, the lighter shaded part the false reports.

reports, and the shaded part the frequency of the false reports. The frequency itself, however, is of less importance than the discrepancies between reactions and reports.

The highest percentage of responses was obtained from the presentation of the individuals' names (A); a relatively large number of correct reports (28·5 per cent. of reactions) were given, and the reports without positive galvanic reaction were not frequent. No essential differences were observed in the three readings of the names. The words 'mother,' 'father,' 'ambitious' as tabulated in (B) give a quite different picture. Here the frequency of the subjective reports equalled or even exceeded the number of the actual reactions and the false reports were more numerous than the correct ones. (C) gives a few sex words: 'kiss,' 'naked body,' 'intercourse.' Though these words elicited reactions in an equally large number of individuals the number of reports varied with each term. The words of (D) express features of character. The reactions are of about the same moderate or low frequency as for the words presented in (B), but the number of both correct and false reports is much lower.

The data presented in the diagram give only a few examples of a circumstance which might be illustrated with any other word of our list, namely, not only that the awareness of emotional reactions was very inexact in the individuals tested but also that it varied according to the topics involved. In addition, it can be shown that the influences which are responsible for the absence or incorrectness of reports are closely connected with social and personal prejudices concerning each individual topic. Reactions to the name, a relatively neutral topic, were reported rather frequently and without numerous mistakes. Such words as 'mother,' 'father' which—according to conventional standards—are expected to arouse affect, elicited few actual reactions but evoked positive reports in many individuals who had shown no galvanic response whatsoever. The many false reports to the word 'ambitious' may be understood on the same basis; the habitual preoccupations of first-year medical students would naturally lead them to expect a reaction to this word. With the words indicating 'unfavourable' features of character (D) we find the opposite tendency. Social standards make it undesirable for the individual to have emotional responses to these words and the reports are correspondingly discrepant. The inhibiting influence of social suggestions on the awareness and on the communication of emotional experiences is very evident also in the diagram with the sex words (C), where the reactions are most frequently reported to the word 'kiss' which presents a situation less subjected to social taboo than the others.

The discussion of the results with the students fully corroborated the above interpretation. The enquiry showed that the errors in the subjective accounts really were in line with the individuals' prejudices concerning the topics indicated by the stimulus words. The overlooking of reactions as well as the reports without adequate foundation corresponded with the subjects' wishes and expectations as suggested by his social standards.

I want to mention that—as other studies showed—the unreliability in the subjective observation and reporting of emotional experiences is not restricted to this group of students. It has been found that the type and the training of the subject does not essentially influence the errors mentioned and that even intimate knowledge of psychological data does not preclude inaccuracy of the reports.

In summary, then, it may be stated again that the individuals' subjective reports of their emotional reactions were not reliable: many marked galvanic reactions were not reported and many positive reports were given where there had been no response. The influence of personal prejudices (social suggestions) was found to be an important if not the determining factor in this incorrect recognition and report of affect responses.

Discussion.

Our findings give definite evidence of the unreliability of subjective accounts of emotional experiences; they also point to the nature of those forces which are responsible for this discrepancy. The question arises, how far a scientific investigation of emotional reactions is handicapped by this unreliability. That the subjective element may invalidate the estimation and evaluation of affect responses has been felt by many workers who have turned their efforts upon those aspects of emotional reactions which seemed to answer best the demand for objectivity. Thus physiological components of the emotions have been extensively examined and their changes during emotional states have been carefully described. But—as Cannon clearly points out—the examination of visceral changes in emotion does not give any information as to the differential features of emotional states; it does not tell us, for instance, whether we have to deal with fear, rage or pain. There have been no conclusive and significant results in this respect from studies on the circulatory and respiratory functions (Weber, Lehmann, Eng, etc.) nor from the work on the psycho-galvanic reflex. Just those differences and finer discriminations of affects which are of greatest interest for the student of the emotional life cannot be elicited through the examination of physiological part-reactions; such differences involve the behaviour of the entire organism and do not lie in one specific component of the complex affect state.

The integration concept, as emphasized in the field of psychobiology especially by Meyer, marks principles which give a valuable orientation in this respect. There are levels of integration (physico-chemical, physiological, psychobiological) which each represent a specific organization of the material involved. Phenomena belonging to one of these integration levels can be treated only by a method adapted to the criteria of that specific level. Thus chemical and physiological methods cannot give adequate information about those phenomena of the emotional reactions which—though including chemical and physiological processes—represent entities of higher organization (function of total organism).

The observation of emotional reactions is complicated—as our findings show—by a social involvement, by an inter-individual factor which cannot be ignored in the study of affects. From the experiments it can be directly concluded that those forces which keep some affect responses from clear awareness and lead to a belief in others that are not there, are a function of personal prejudices and social standards. The important rôle of this source of errors in the observation and evaluation of emotions has been emphasized by Burrow who points out that social suggestions quite generally handicap and distort not only the recognition of one's own emotional reactions but also one's examination of the affect states in others. A method also has been outlined by this author (consensual analysis in groups) which takes up definitely the investigation of this social factor as present in both the act of observation and the emotion observed. This social factor cannot be adequately examined from the standpoint of the individual entity alone; it needs to be approached by a procedure which includes the social integration level. A method adapted to manifestations of this inter-individual level offers the possibility of investigating more closely such social involvements as are expressed in the discrepancies found between emotional reactions and reports.

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A METHOD FOR MEASURING SIMULTANEOUS CONTRAST

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Two objects of unequal colour mutually influence each other. For more than a century this phenomenon has been known as simultaneous contrast. Various theories were advanced as possible explanations, the first of which classified it under 'optical illusions.' Soon, however, the important biological significance of this process became evident. Indeed, the primary stimulus here not only exerts a direct influence on the spot where it acts, but it also makes itself felt indirectly, and more particularly in an opposite way, on contiguous elements.

Even more important becomes this significance when it is subjected to a closer scrutiny. That is to say, it has a restraining effect on the disturbing influence that asserts itself whenever the stimulation of a definite area necessarily causes an accompanying excitation of contiguous parts in an identical way. This causes the simultaneous contrast to be viewed in a totally different light. Indeed, it now becomes a physiological correction of the imperfections of the dioptrical apparatus it establishes "eine physiologische Stigmatik, trotz dioptrischen Astigmatismus."

A third—purely psychological—theory of simultaneous contrast was formulated by Helmholtz and von Brücke. According to them it was founded on an error of judgment; a change in the subjective evaluation of the norm of what we call 'white.'

Helmholtz and von Brücke assert that in the phenomena of simultaneous contrast sensation remains unchanged, but owing to an erroneous judgment it is interpreted differently and thus leads to a different representation.

The apparent dimensions of an object depend on the brightness and darkness of that object and of its surroundings. Irradiation enters in and first and foremost is caused by the physical aberration of light, through imperfections of the optical system. In the phenomenon of irradiation the defectiveness of the optical apparatus discloses itself; this is but partly compensated by simultaneous contrast. Here we have undoubtedly the oldest conception.

Thereafter came a period in which Plateau, and many with him, adhered to a purely physiological theory of irradiation. It was thought that a purely physiological process caused an accompanying stimulation in the contiguous elements in an identical manner, *i.e.* precisely the opposite to simultaneous contrast. Plateau came to this theory by virtue of the so-called Laws of Irradiation. He found the following data in his endeavours to arrive at some method for measuring irradiation:

(i) Increase of the apparent dimensions by irradiation is independent of the visual angle under which the object is viewed, provided the angle is not too small.

(ii) The apparent dimensions increase with the intensity of the light.

Plateau is quite justified in remarking that an explanation, which supposes a change in the distribution of light in the eye merely as a result of increase in the intensity of the light, is untenable. Hence his rejection of the physical theory of irradiation and substitution of a physiological one. In this, however, he entirely overlooked the fact that increase in the dimensions of the irradiation phenomena may be attributed chiefly to an insufficiency of the simultaneous contrast effect.

As mentioned before, together with a local excitation an accompanying influence begins to assert itself on the contiguous elements in an opposing sense. This causes a secondary augmentation of the component 'black' in these elements, and the latter in its turn has a thwarting effect upon the sensation 'white,' which in those contiguous elements is primarily excited by the aberration of light. The contrasting black compensates, within certain limits, this light sensation and in this way reduces the area of aberration.

But at the same time the light sensation, on account of contrast, is relatively stronger on the borders of the stimulated area than at its centre. This causes it to appear as if objectively a stronger light stimulus was present at the contours. The diminution of the area of aberration is, therefore, likewise only to a certain extent compensated. Summarily stated, we obtain an inward displacement of the limits for black, and an outward movement of the borders for white. The outcome of all this is the formation of a limit within the area of aberration. Under these circumstances Plateau's explanation was not quite correct. He observed rightly that here we were face to face with a process that did not admit of a purely physical interpretation, but required further physiological elucidation. He overlooked, however, the influence in this matter of simultaneous contrast, and was, therefore, compelled to arrive at a theory of physiological irradiation.

A number of experimenters have endeavoured, in various ways, to find physiological explanations of irradiation phenomena; it would be beyond the compass of this article to quote them here.

In my opinion the present conception of the problem is to view the so-called irradiation phenomena as the resultant of two component factors, viz. physical aberrations of light and simultaneous contrast, or rather, those sensations which we perceive subjectively as 'irradiation phenomena' are in reality caused by the aberration of light, but are partly corrected by simultaneous contrast. In my investigations I proceeded from the experiments made by Plateau. In order to measure the extent of irradiation phenomena Plateau made use of the apparent confluence of two white squares on a black background. By moving one of those squares in a diagonal direction it was possible to obtain a position in which the edges of the squares appeared to lie in each other's projection. The degree to which a square had to be moved in order to procure the desired effect served as a measure for irradiation.

In repeating these experiments I introduced the following modifications. In order to make the phenomena as distinct as possible and, therefore, as observable as possible, I made use of a pencil of light thrown on a dead-silver screen in a perfectly dark room, instead of a small white square on a black background. This pencil of light was procured by introducing a metal lamina above the large lens of the epidiascope of Zeiss; in this lamina a square aperture of 1 cm. was made.

By this arrangement it becomes possible simply by focusing sharply or not to project a clearly or vaguely defined square of white light on a neutral dark background. If in this metal sheet another square aperture of the same dimensions be made in such a position relative to the first opening that the two diagonals, touching each other at one angle, lie in each other's projection, then, according to Plateau, the squares will move over one another at the point of contact, and in this way form a confluence. It was, therefore, surmised that an illuminating bridge was being struck between the two lighting squares, and in this way they would pass into each other. No matter how I tried to observe this phenomenon I did not succeed.

A few of my intelligent observers who were subjected to this experiment noticed an illuminating bridge between the two squares, but at the same time there were others who did not observe this. On perusal of the literature on the subject it appeared that le Roux[1] had a similar experience. He mentions that all phenomena of irradiation are hardly noticeable to his vision on fixation, *i.e.* when looking with the macula lutea. Looking, however, indirectly, with the periphery the phenomena became more distinct. And on scrutinizing Plateau's publication carefully it appears that he also was troubled by these experiences. Indeed, he states that irradiation phenomena are more distinct with vague focusing and advises the introduction of a spherical positive lens before the observer's eye. Evidently Plateau also found the 'irradiation phenomena' only distinct when the image was not sharply outlined.

Whenever I refrain from fixing my eyes on the image formed by the lighted square, but look at it with a peripheral portion of the retina, I can distinctly observe the phenomena by which the lighted areas become confluent. These phenomena appear even more beautifully, and are distinctly demonstrable whenever we change the adjustment of the epidiascope in order to obtain a hazy outline of the squares. The hazier the outline, the larger the extent to which the areas become confluent. This, naturally, compelled Plateau to introduce positive lenses before the eyes of his observers.

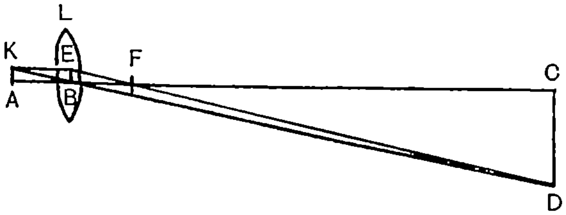

Fig. 1.

In the above diagram KA represents a cross-section of the square aperture in the metal lamina, introduced above the large lens of the epidiascope; therefore EB, which is equal to KA, is 10 mm.

At L the system of lenses of the epidiascope is placed, the focus of which lies at F; distance BF amounts to (measured) 250 mm. Exact focusing of the epidiascope projects a sharply outlined image on a dead-silver screen placed at some distance. The dimension of this image $= CD$; the distance from the epidiascope to the screen, on being measured, proves to be 5075 mm. Therefore $CF = 4825$ because $BF = 250$. As

$$\triangle EBF \sim \triangle DCF,$$

$$\frac{EB}{BF}=\frac{CD}{CF} \text{ or } \frac{10}{250}=\frac{x}{4825}. \quad \therefore x = 193.$$

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[1] F. P. le Roux, "Sur l'Irradiation," *Compt. rend.* 1873, LXXVI, 960–962.

Consequently the dimension of a sharply outlined square on the screen = 193 mm. On actually measuring this sharply outlined image projected after exact focusing we obtained 194 mm.

This proves that though our method of calculation is very simple, serious errors have been avoided. We went further, and calculated the dimensions of the hazy image, projected on the screen after vague adjustment. This calculation can also be performed in a simple manner.

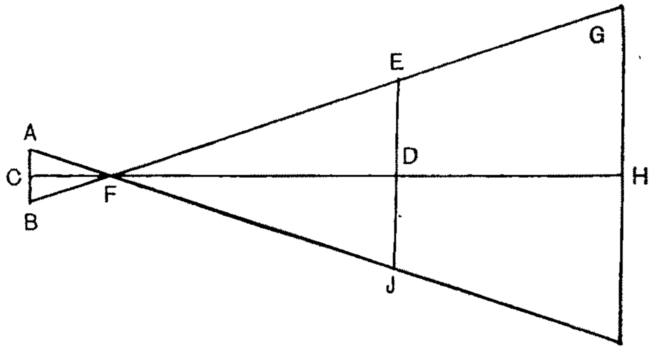

Fig. 2.

Let AB represent opening of lens diaphragm; $CB = \frac{1}{2}AB$. On turning the large lens of the epidiascope downward through a certain distance, we obtain an indistinct image on the screen. At a nearer distance, *i.e.* distance CD, a sharply outlined image is produced. CD is now measured.

The outermost radius of this image is BEG. $\triangle BCF \sim \triangle EDF$, therefore $FD : FC = ED : BC$.

Distance CD is known, therefore CF is known, and also FH; and as $GH : BC = HF : FC$ we also know GH.

It was, thus, a simple matter to calculate the dimensions the lighted square should have at various degrees of haziness. However, on comparing these values with the figures obtained by actual measurement striking differences became evident. Without exception the values arrived at by calculation were found to be higher than those obtained by measurement and the more vague the focusing the more striking the differences.

At once, however, we should draw attention to the very real difficulty of measuring the dimensions of an illuminated area with such hazy outline. To arrive at figures that could boast of even a moderate degree of correctness is practically impossible. But no matter how difficult this measuring might be, it was quite evident that the results did not tally with those obtained by calculation. On looking at the illuminated area we found complete darkness, where, according to our calculation, the line of demarcation should have been found. Our first thought was that our calculations must be wrong. But it was a simple matter to photograph the illuminated area and to compare this photograph with our subjective sensation of the same area. This was done and the photograph proved conclusively the correctness of our preliminary calculations.

Below we mention some of the results; the dimension of the illuminated area in cross-section, after sharply focusing the epidiascope, was calculated at 193 mm. By placing a small ruler on the screen the cross-section proved to be 194 mm.; photographed 193 mm. On turning the face of the epidiascope, in which the larger lens is placed, 1 cm. downward, we obtained the following values for the cross-section:

Calculated	207 mm.
Measured	200 ,,
Photographed	205 ,,

In turning down the face with the large lens through a distance of 2 cm. we obtained the following values for the cross-section:

Calculated	223 mm.
Measured	210 ,,
Photographed	220 ,,

On turning the face of the epidiascope, in which the large lens is placed, through a distance of 3 cm. we obtained linear measurements for the hazily outlined square:

Calculated	246 mm.
Measured	225 ,,
Photographed	240 ,,

These measurements were repeated about 10 times on different days. Results as under:

Commencing with the two constants, *i.e.* the values obtained by calculation and photography, we have (values in mm.):

Face of epidiascope in which lens is placed turned down	Calculated	Photographed
1 cm.	207	205
2 ,,	223	220
3 ,,	246	240

Values obtained by measurement, also in mm., diverge somewhat considerably, which is, however, quite conceivable in view of the difficulties encountered here.

Date of observation	Face of epidiascope in which lens is placed turned down		
	1 cm.	2 cm.	3 cm.
1	198	220	223
2	194	198	243
3	222	206	227
4	204	234	235
5	194	202	201
6	198	199	240
7	196	200	232
8	210	236	236
9	192	204	224
10	202	214	227
Average of 10 observations	201	211	228

To enable us to investigate matters as accurately as possible, we sorely felt the need of a rapid and reliable method of measuring at once, and successively, the dimensions of the illuminated area as it presented itself in reality, in a physical sense, and as it is observed by us subjectively. For the sake of greater brevity I shall call these the objective and subjective dimensions of the area.

After some trials we adopted the following method of measuring the objective dimensions of the illuminated area, *i.e.* the dimensions as they are recorded by photography.

A metal frame of 44 × 35 cm. is placed on a stand (Fig. 3). In this frame

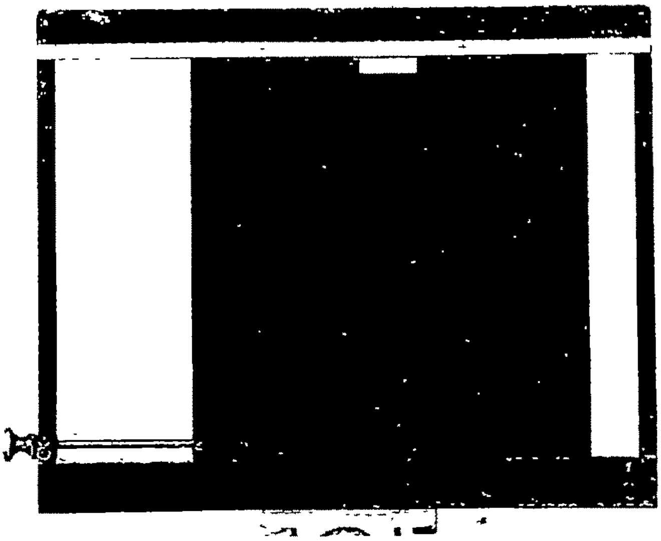

Fig. 3.

a metal slide of 34 × 28 cm. is adjusted. This slide, attached to a screw, can move to and fro within the frame, and can, therefore, pass through a distance of 16 cm. from right to left and *vice versa*. The very fine thread of the screw allows of a minute adjustment of the sliding motion. A scale in mm. enables us to take readings of the position of the slide within the frame, directly, to ½ mm. correct. In the centre of this slide, which is made of ¾ mm. metal, there is an aperture of 1 mm. diameter. The procedure is as follows:

The instrument is set up in the same position as was originally occupied by the screen on which the illuminated area was projected. The distance from the epidiascope is, therefore, the same as in our first observations. The observer now takes up his position behind the metal slide and looks through the tiny aperture in its centre. His eye is struck by the glaring pencil of light emanating from the carbon lamp of the epidiascope. He now turns the screw mechanism, so that the slide and its aperture move to the right. At first, on looking through the small aperture, the glaring pencil of light remains in view, but when the aperture gradually approaches the limits of the illuminated area, the pencil of light begins to disappear, just as the illuminated fundus oculi passes out of view in conducting sciascopic measurements. The moment in which the pencil of light disappears entirely can be observed with absolute accuracy. The observer can still see light distinctly, when the direct pencil has disappeared, but there is such an enormous difference in intensity between this diffused light and the direct ray from the arc lamp that nobody could possibly be in doubt.

In this way it is possible to define the exact moment of transition from light to darkness on the one side of the lighted square. The whole apparatus with its stand is set up on a board. This board can slide along a lath on the floor, which is provided with a scale in cm. This lath has been fastened so as to enable the apparatus to be moved parallel to the screen which we had in that same position originally. Now, *e.g.*, after having carefully determined the limits on the right side of the square, and after having taken the readings on the mm. scale on the edge of the frame, as well as those on the cm. scale of the lath on the floor, we slide the whole apparatus with the board, on which it is fastened, along the floor lath, and move so far to the right that we approach the vicinity of the left limit of the illuminated square. We again adjust carefully with the aid of the mm. scale, and take readings on both scales. In this manner the width of the illuminated area can be measured in mm. exactly.

By this method I have determined the objective dimensions of the small lighted area, when sharply focused, and at various degrees of hazy adjustment. Results as under:

	Cross-section of illuminated area in mm.
(*a*) Focused exactly	193
(*b*) Hazy adjustment (face of lens 1 cm. lower)	205
(*c*) „ („ 2 cm. „)	220
(*d*) „ („ 3 cm. „)	240

On comparing these values in the following table with those obtained by photographing the illuminated area, we observe:

Cross-section of illuminated area in mm.	Measured with perforated sheet	Photographed
(*a*) Focused exactly	193	193
(*b*) Face of lens 1 cm. lower	205	205
(*c*) „ 2 cm. „	220·5	220
(*d*) „ 3 cm. „	240·5	240

These values show conclusively the usefulness and exactness of this method. The sharp outlines that can be obtained make it preferable to photography. One definite point can be minutely determined, and, thus, we arrive at a norm for further comparative measurements. But a still greater advantage is the simplicity and quickness with which we can again and again obtain new series of measurements, before there is any considerable change in the light intensity of the arc lamp in the meantime.

It was more difficult to find a sound method for determining the subjective value of the illuminated area. After repeated failures the following arrangement, analogous to Plateau's measurements of irradiation, appeared to solve the problem (Fig. 4).

The epidiascope has some additional metal discs provided in the centre with a square opening, in which lantern slides can be fitted for projection. For one of these round discs we obtained a covering plate *ABCD*, which could be fitted lightproof on it by means of two small hooks *P* and *Q*. In the centre of this plate *ABCD* there is an opening which can best be described as a square of 4 square cm., and which is divided into four equal squares of which the lower one is left in. Two of the three square openings that constitute the larger perforation are, therefore, situated in such a position that one of them has a diagonal lying in the projection of one of the diagonals of the other square; in Fig. 4 the horizontal diagonal; the third square fits in between the other two, so as to have one of its diagonals vertically at right angles to the two diagonals referred to above.

On the square *ABCD* a brass rod *E* is adjusted, which by a screw at *F* can be moved in the direction of the vertical diagonal. This rod carries at its one end a small metal square *G* which, consequently, can be moved upward and downward, thus covering up the opening to a greater or lesser degree.

As the whole apparatus is very carefully finished, it is possible to screw the little square *G* so far downward as to cover up exactly the centre square of the opening, in this way leaving only two small squares open, which touch each other at one angle only. By sliding down the rod with square *G* further still, the two remaining apertures become more and more separated.

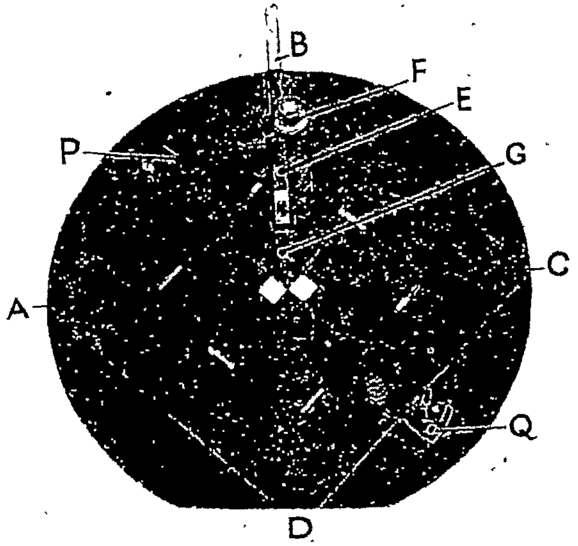

Fig. 4.

This apparatus is now placed in the epidiascope and initially the square plate *G* is screwed down so far that two square apertures, each of 1 × 1 cm. and situated diagonally and horizontally opposite to each other, remain open. The pencil of light of the epidiascope is again projected on a dead-silver screen in a dark room and sharply focused. In this way we procure two brilliantly illuminated areas, which touch each other at one angle. The face of the epidiascope, in which the large lens is placed, is now turned downward. Consequently we obtain a hazy projection, the 'irradiation phenomena' appear, and the two lighted areas become confluent. A bridge of white must, therefore, have been struck between the two lighted areas and their side lines no longer lie in each other's projection. By manipulating screw *F* we are enabled to move plate *G* in such a manner, and so far down that it appears to the observer, who stands near the epidiascope, as if the two white areas again touch each other at one angle exactly, and their side lines again coincide with each other's projection. On account of the considerable distance between the observer and the screen it becomes possible to indicate the position the square *G* should take up, so as to bring the two white areas just in touch with each other; with some practice a considerable degree of accuracy can be obtained.

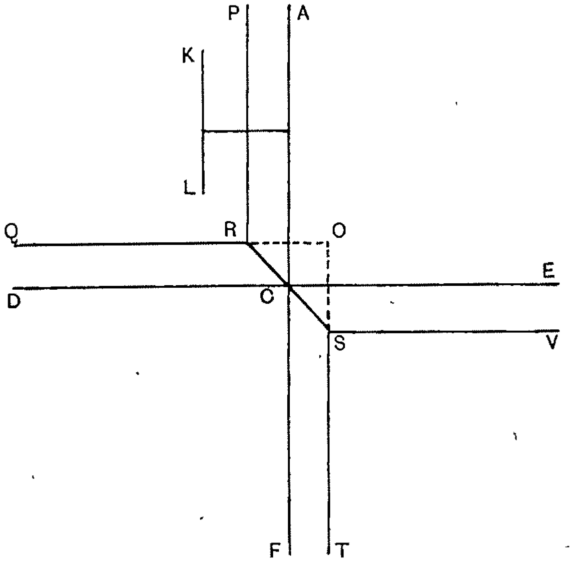

Fig. 5.

As soon as we have reached this adjustment, we again focus the epidiascope sharply. But now it appears that we have a dark area between the two light squares; by turning plate *G* downward we have separated the two light squares. They are now situated at some distance from each other. This distance is a straight line, drawn from the right-hand angle of the light square on the left, to the left-hand angle of the light square on the right, and can be measured directly by applying a ruler. In this value we have a measure for irradiation.

Referring to Fig. 5, we have with hazy adjustment turned the screw in such a manner that angle *ACD* of the left-hand hazy square appeared just to touch angle *ECF* of the right-hand square at *C*. Whenever we focus sharply on the screen, the lighted squares appear to be drawn asunder, the hazy precincts become sharply outlined, $\angle ACD$ becomes

$\angle PRQ$ and $\angle ECF$ becomes $\angle VST$. The distance RS now provides a measure for irradiation. Suppose the distance between PR and $AC = \frac{1}{2}p$ and $RS = x$, then it follows that $x = p\sqrt{2}$. In this way we can measure the distance RS at various degrees of haziness and hence we can obtain the dimensions of the illuminated square at various degrees of hazy adjustment.

The clear line of demarcation PR becomes, with hazy adjustment, a band of width $\frac{1}{2}p$. The original border line must be situated in the centre of this band. On each side, therefore, is added a hazy strip of $\frac{1}{2}p$. Consequently length and breadth of the lighted square are being enlarged with a strip of width p. In order, therefore, to arrive at the dimensions of the whole hazy square we need only add to the dimensions obtained from the cross-section of the sharply outlined square the value of p. This value of p depends on the degree of haziness and is obtained for each of these adjustments by calculation as above. By these two methods I have, therefore, measured the subjective and objective values of the dimensions of a small illuminated square. Precautions were duly taken to ascertain a possible change in the intensity of the light, which is regulated by the parabolic mirror in the epidiascope. To this end the intensity of the illumination of the area was regularly measured with a photometer of an extremely simple construction (see Fig. 6).

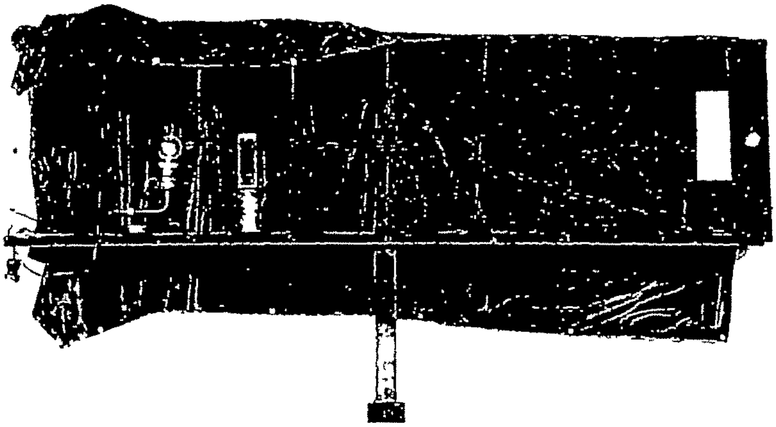

Fig. 6.

A horizontal board of 2 m. length is fastened to a stand. At mutual distances of 10 cm. a number of small metal arches are stretched over this board, which serve as supports for a large black cloth. This cloth also covers one of the ends of the board, but on the other side it terminates in a small wooden case. The front of this case is perforated by one round hole of 3 cm. diameter. Behind this aperture a glass plate is inserted obliquely at an angle of 45°. This plate is silver-coated and, therefore, presents the same aspect as a silver projection screen. A movable 25 c.p. electric lamp, attached to a stand, is placed under the black cloth on the board, which is provided with a scale in cm. Between the lamp and the silver mirror, and vertically to the board, a vessel is placed, which is filled with a coloured liquid, consisting of indigo blue and malachite green in such proportions as to change the yellow light of the electric lamp to the same colour as the light of the arc lamp in the epidiascope. Subsequently we place a metal sheet in the epidiascope before each observation. In this sheet an aperture has been made in such a manner that a round illuminated area of 3 cm. diameter is projected on the screen. We now place the photometer in a position adjacent to the projection screen and at such an elevation as to have the round opening in the front of the little case and the circular spot of light on the screen on the same level and near to each other. In this way they can be easily compared. By simply moving the source of light in the photometer backward or forward in accordance with the greater or lesser intensity of the light emitted by the epidiascope, we have a simple method of comparison.

Although we have refrained from mentioning this in each particular instance, we have by this method taken precautions, during the course of the following measurements, to maintain constancy in the intensity of light. In order to enable us to deal with these values in a more facile manner, we proceed as follows: let a^2 be the dimension of the sharply outlined square, and b^2 the dimension of the vaguely bordered squares, as they are seen subjectively, while c^2 would be the objective dimension.

We take b to represent the subjective value of the measure of length of the hazily outlined square. b_1 represents this same length with the face of the epidiascope, in which the large lens is placed, turned 1 cm. downward; b_2 after a downward movement of 2 cm., etc. These values are determined by our method for ascertaining subjective measurements described above.

The objective values of the measure of length of the hazy square are represented by c. Then c_1 represents this same value with the face of the epidiascope, in which the large lens is placed, turned 1 cm. downward; c_2 at a downward turn of 2 cm., etc. These values were found by our method for determining objective values described above. Therefore $b_1 = a + p_1$ and the value of p_1 follows from the equation $x_1 = p_1\sqrt{2}$. In this p_1 and x_1 are the values for p and x by hazy adjustment of the illuminated square, obtained by turning the face of the epidiascope, in which the lens is placed, 1 cm. downward. Consequently, we denote the values for a hazy adjustment, obtained by a downward movement of 2 cm., etc., by p_2 and x_2, etc. Some of the results obtained were as follows:

Date of observation	x_1	x_2	x_3
1	11	21	38
2	11	20	39
3	10	20	39
4	9	22	40
5	10	20	41
6	9	20	40
7	11	21	39
8	10	24	42
9	9	23	41
10	10	21	42
11	9	20	40
12	11	21	38
13	9	19	41
14	10	21	41
15	9	22	41
16	11	20	39
17	10	21	40
18	10	22	40
19	10	20	38
20	11	21	41
Average of 20 observations	10	21	40

We now have:

$$x_1 = 10, \therefore p_1 = 3{\cdot}5 \text{ and } b_1 = 196{\cdot}5.$$

$$x_2 = 21, \therefore p_2 = 7{\cdot}4 \text{ and } b_2 = 200{\cdot}4.$$

$$x_3 = 40, \therefore p_3 = 14{\cdot}1 \text{ and } b_3 = 207{\cdot}1.$$

The dimension of c at various degrees of haziness under constant illumination is always found to have the same value, viz. $c_1 = 205$; $c_2 = 220$; $c_3 = 240$.

In this way we have succeeded in measuring the 'subjective' and 'objective' dimensions of a vaguely outlined lighted square at various degrees of haziness.

This haziness occurs principally through the aberration of light. This phenomenon is expressed in the values found for c. There exists, however, a correction for this haziness in our eye, viz. the simultaneous contrast. On account of this adjustment in our eye we interpret those figures differently, *i.e.* smaller and more clearly outlined, than they are in reality. The result of this correction is expressed in the values found for *b*. But then the difference between *c* and *b* provides us with a measure for the simultaneous contrast. Consequently it has now become possible to measure simultaneous contrast and express this in number.

Availing myself of this method I intend to perform measurements of simultaneous contrasts under a variety of circumstances, changing intensity of illumination, etc.

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A SUGGESTION TOWARDS A NEW HYPOTHESIS REGARDING THE LOCALIZATION OF SOUND

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I. *Object of the Paper* (pp. 142, 143):
II. *Experimental Facts which any Hypothesis of Localization must explain* (pp. 143, 144).
III. *Physiological Basis of the Hypothesis* (pp. 144, 145).
IV. *The Assumption on which the Hypothesis rests* (p. 145).
V. *A Possible Criticism* (pp. 145, 146).
VI. *The Hypothesis* (pp. 146–152).
VII. *The Length of the Refractory Period* (p. 152).
VIII. *Summary* (pp. 152, 153).

I. *Object of the Paper.*

1. ONE of the greatest stumbling-blocks to any theory of hearing is the problem of the localization of sources of sound.

2. Many hypotheses have been put forward to explain how judgments of the direction of the source of a sound are made, but towards the end of the last century it was usually supposed that discrimination between right and left was due to the greater intensity of the sound at the ear which was nearer the source[1]. About that time Rayleigh (12) pointed out that the intensities at the two ears were not appreciably different for pure notes of low pitch, owing to the diffraction of the sound waves round the head; yet the source of a low-pitched sound could be indicated. This fact was still explained on the intensity hypothesis for, owing to the difficulty of obtaining pure tones, which probably never occur in nature, it was assumed that the higher overtones of the sound affect the nearer ear only, and thus produce the necessary difference of intensity. Little experimental investigation was undertaken till about 1907, when Rayleigh (13) suggested, as a result of his observations, that the localization

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[1] It might be thought that in normal life localization is decided by which ear is first stimulated. This holds for single clicks, but not for continuous sounds. If the intensities at the ears are equal, a continuous sound of low pitch is localized to the side which leads in phase, even though the ear at which the phase lags is first stimulated (3). Similarly a note of high pitch is localized to the side at which the intensity is the greatest.

of low notes depended on the appreciation of binaural differences of phase. In the following year Wilson and Myers's experiments were published (15). Wilson and Myers found that the localization of sounds of low pitch depended on the difference of phase at the two ears, and further claimed that the phase difference at the ears was in effect an intensity difference. It has since been shown that the assumptions they made are not justified, and that their explanation does not hold ((3), (4)). In the same year, 1908, Bowlker also published experimental results (6). These indicated that notes of low pitch were localized by the difference of time between the arrival of similar parts of the same wave at the two ears[1].

3. These hypotheses explain localization simply by stating the physical factors on which localization depends. This paper is an attempt to formulate a hypothesis which will advance the theory one step further. It knits together the various factors which otherwise appear to be, in a most disjointed manner, the physical basis of sound localization, and aims at showing how the physical factors are related to the physiological factors. No attempt is made to formulate any mechanism for the further relation between the physiological and the psychological factors.

II. *Experimental Facts which any Hypothesis of Localization must explain.*

4. The experimental facts which require explanation are, very generally stated, as follows:

(i) A short sound (click) is localized by the time-difference between the arrival of the sound at the two ears ((9), (10), (11), (16), and this paper, p. 147);

(ii) Continuous sounds of low pitch are localized by time-difference, *i.e.* by the time-difference between the arrival of similar phases of the sound waves at the two ears[2] ((6), (14));

(iii) Time-difference gives ambiguous localization with continuous sounds of high pitch ((1), (14), (15));

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[1] Bowlker did not, however, suggest that localization was due to time-difference. He ascribed the effect to phase-difference, but ((6) p. 330) noted that if a source of sound were 2·4 in. further from one ear than from the other, the sound was localized in the same direction with frequencies varying from 327 to 2045 d.v./sec., *i.e.* the localization depended on the time-difference.

[2] In spite of the results of the experiments I published in 1925 (1) I consider that localization by phase-difference is a special case of localization by time-difference. I shall deal with this question in a later paper.

(iv) With a continuous sound of high pitch and equal physical intensity at the two ears two (or more) apparent sound sources can be localized ((6), (14));

(v) Localization is ambiguous with sounds of low pitch if the time-difference is made greater than a certain maximum (9);

(vi) Continuous sounds of high pitch are localized normally by intensity-difference, *i.e.* towards the side at which the intensity is the greatest;

(vii) Time-differences cannot be neutralized by intensity-differences. If a sound of low pitch is localized to one side because of time-difference, the position of this sound is not affected by increasing (within limits) the intensity at the far side ((2), (7), (10));

(viii) With sounds of low pitch two apparent sources can be localized when the intensities at the ears are unequal ((2), (14));

(ix) While intensity-differences are effective with sounds of high pitch they have little effect on the localization of sounds of low pitch ((2), (14), (15));

(x) A continuous sound of constant (physical) intensity may vary in pitch with its localization, the intensities at the two ears being equal throughout ((10), and this paper, p. 147).

5. In addition to these facts, a complete psychological theory should explain changes in the volume of sounds, why the plane in which a sound appears to move varies with the individual and with the sound, and why some individuals localize sounds inside the head while others project them outside. I do not, however, propose to deal with these matters in this paper.

III. *Physiological Basis of the Hypothesis.*

6. Although the details of the mechanism of the ear are not fully established it is generally conceded that the energy from sound waves which impinge on the tympanum is transmitted through the middle ear to the cochlea and stimulates the hair cells of the organ of Corti, which are the sensory elements. The nervous impulses which are produced in these hair cells travel along the auditory nerve and arouse our perceptions of sound. It is also generally conceded that the nerve impulses are not of the form of a continuous flow of energy. They consist of discrete impulses. After each impulse has passed there is a certain definite time during which the passage of another impulse is inhibited (the Refractory Period).

7. There is a difference of opinion among physiologists as to the

exact mode of excitation of the hair cells, the cilia of which appear to be embedded in the tectoral membrane. It seems probable, however, that the nervous impulses are originated by some displacement of the cilia with reference to the hair cells. The cilia, it is agreed, go through a cycle of motion relatively to the hair cells, and thus arouse the nervous impulses.

IV. *The Assumption on which the Hypothesis rests.*

8. It is at this point that I wish to make an assumption: that the nervous impulse in the auditory nerve fibre is initiated when the cilia are in a certain phase of their cycle. This is pure assumption, but I make it because it is essential to my hypothesis. Our present knowledge of the functioning of the hair cells does not, I believe, make the assumption untenable.

V. *A Possible Criticism.*

9. Yet there is one criticism which has to be met, viz. it is found that the rate at which nervous impulses may follow one another along a nerve varies with the strength of the exciting stimulus. Why should this not occur in the case of the auditory nerve fibres?

10. This is a real difficulty. It is obvious that the assumption I make breaks down if the rate of discharge of the nervous impulses in the auditory nerve fibres varies with the intensity of the external stimulus, because then the discharges will not occur when the cilia are in a certain phase of their cycle. For all sensory nerves so far studied the rate of discharge of nervous impulses does vary with the intensity of the stimulus and there is no adequate reason for assuming that the auditory nerve fibres will be found to behave in any other than the normal manner. There is only one way in which it is possible for a constant rate of discharge of the nervous impulses to be maintained when the external stimulus varies in strength—the constant external stimulus must be transformed into a periodical stimulation of the sensory nerve endings. This is exactly what does appear to take place in the ear.

11. Though there is much dispute as to the exact mode of excitation of the nervous impulses, Hartridge's view (8) is perhaps the most probable. As I have already indicated the general opinion is that the ends of the cilia are embedded in the tectoral membrane, which is of a jelly-like structure. When the basilar membrane is in vibration and is moving towards the tectoral membrane, the hair cells push the tectoral membrane bodily upwards (Fig. 1 (i)). This cannot, in Hartridge's opinion, originate any impulse, since the cilia are not affected. But, when the

basilar membrane moves away from the tectoral membrane (Fig. 1 (ii)), the latter, having a comparatively large mass, continues in its motion till the cilia pull it down. This tension on the cilia originates the nervous impulse. Hence it seems probable that when the ear is stimulated in the normal way by continuous sound waves, the auditory nerve cannot be called upon to transmit nervous impulses at a rate faster than the frequency of the particular sound wave, whatever the intensity of the sound may be. This does not mean that the nerve cannot transmit impulses more frequently if, experimentally or otherwise, strong *continuous* stimuli are applied.

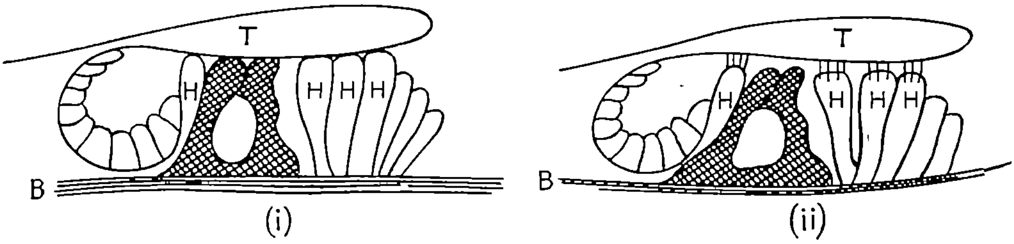

Fig. 1. *Diagram to show the probable manner in which the nervous impulses are originated.* For the half vibration during which the basilar membrane (B) is moving towards the tectoral membrane (T) [(i)] the cilia of the hair cells (H) are not under tension. There is tension on the cilia when the basilar membrane (B) is moving away from the tectoral membrane (T) and is dragging the tectoral membrane after it [(ii)].

VI. *The Hypothesis.*

12. Now let us take the simplest possible theoretical case, and consider what will happen, granting the above assumption, if two short clicks of equal intensity are produced, one at either ear.

13. If the auditory mechanisms are alike, the time-difference between the arrival, at the corresponding sensory areas of the brain, of the nerve impulses from the cochlea may be assumed to be equal to the time-difference between the sounds at the tympana. What occurs after the excitation of the sensory areas is pure conjecture, but the excitations are passed on. They may, or they may not, be perceived consciously. If they are not perceived consciously they go to form part of the general background. (I would compare the effect to that of the clock of whose ticking we are unaware till it stops.) If they are perceived consciously they may be perceived as two separate sounds, or they may be perceived as a single sound. This is illustrated in experiments as follows: Two very

short clicks are made, one at either ear of an observer. The apparatus is so arranged that any desired time interval can be given between the clicks. When the clicks are simultaneous one sound is heard. It is localized in the median plane. If a short interval occurs between the two clicks one sound is still heard, but it is localized to the side which is first stimulated. As the time interval is further increased the sound appears to move more and more towards the side which is stimulated first, till, at a certain time-difference between the clicks, there is much uncertainty whether one sound is heard or whether there are not two sounds, one on either side, the second being much quieter than the first. A further increase of the time-difference gives two sounds of equal loudness, one to the right, the other to the left. In these experiments the two clicks are not heard separately when they follow one another closely; instead they are perceived as one sound with a certain localization. My observers recognized the binaural sound as being the same sound whatever its localization. It did not vary in loudness, though it varied in position, and sometimes in volume.

14. It is impossible at present to say how this factor of localization is finally decided, but the first impulse has a certain priority over the second, and it is this priority which determines our judgment of the direction from which the sound appears to come[1]. The effect is not analogous to the inhibition of one impulse by a previous one which has passed along the same nerve fibre. In this case, if the second impulse follows the first within the Refractory Period it is denied passage. This does not occur when the impulses are along corresponding fibres from the two cochlea. Such impulses are integrated, and the result of the integration gives the sound, among other things, this new attribute of varying direction (Fact I).

15. Let us now apply this argument to the case of a pure tone, assuming some form of Resonance Theory and that it is possible for a pure sinusoidal wave to get through to the cochlea fluids without transformation. Provided the vibrations are sufficiently great the cilia will be in the position at which a nervous impulse can be aroused (the 'effective position') once in each vibration, and if the wave at one ear

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[1] The priority is further illustrated in the case of continuous sounds by those observers who judge a given note to be higher in pitch when heard with one ear than when heard with the other. One of my observers exhibited this phenomenon strongly. When his ears were stimulated separately with a note of a given pitch one sound was heard having a certain localization depending on the time-difference. The variation in the localization was accompanied by a corresponding variation in pitch. The pitch is determined by the priority (Fact X).

is in advance of that at the other we have all the factors necessary to enable us to judge the direction from which the sound comes (Fact II).

16. There are, however, two limits to this:

(*a*) the frequency of the vibrations of the basilar membrane, and therefore of the sound waves, must not be so great that the cilia return to their 'effective position' during the Refractory Period; and

(*b*) the two waves coming to the two ears must not be separated by such a time interval that two sounds are discriminated.

We will consider these two limitations separately.

17. If the rate of vibration of the basilar membrane is so high that the cilia are in the position for originating a second impulse in the nerve fibre during the Refractory Period following a previous impulse, this second impulse will be inhibited. For example, suppose the Refractory Period of the auditory nerve fibres is 2σ, then the rate at which successive impulses may follow one another cannot be greater than 500 per sec. If a note of frequency 750 d.v./sec. falls on one ear the cilia will be in the 'effective position' 750 times each second. So if one nervous impulse has been initiated the next nervous impulse will be initiated every other time the cilia are in the 'effective position,' *i.e.* the impulses will follow one another at the rate of 375 per sec. Similarly for the other ear. Now if the same sound wave initiates the nervous impulse in each ear, *i.e.* if the impulses are 'in step,' all the data for making a correct judgment of direction are present. But, if alternate waves initiate the impulses in the two ears, *i.e.* if the wave which is operative at one ear is inoperative at the other, so that the impulses are 'out of step,' the data will not give a correct judgment of direction. Now there is no certainty that the nervous impulses will be 'in step'; in fact since the two ears are separately excited it would appear to be largely a matter of chance whether the data will cause a correct judgment of direction to be made in the absence of other determining factors, that is localization will be inconsistent. This inconsistency will be much more marked when the pitch is so high that the cilia are in their 'effective position' three (or more) times during the Refractory Period. The sound source may then appear to have any of three (or more) different positions, so it is to be expected that localization by time-difference will be inaccurate at these high frequencies (Fact III).

18. Let us consider this matter a little further. Suppose the cilia are in the 'effective position' for initiating a nervous impulse three times within the Refractory Period of the nerve. Then only one of three successive vibrations (*a*, *b*, *c*) can initiate the nervous impulse from one ear:

similarly only one of the three vibrations (α, β, γ) can initiate the impulse at the other ear. Unless there is some predisposing cause there would seem to be no good reason why the impulse from the vibration (b) should be paired with the impulse from the 'in step' vibration (β) rather than with that from (α) or (γ), *i.e.* why the sound should be judged to come from any one of three possible directions. In fact it would seem possible that, even if one direction were favoured initially, a moment later one of the other directions might have the advantage owing to some one of many physical, physiological and psychological causes. One would not, therefore, be surprised if, with such a note and with equal intensities at the ears, three separate sound sources were localized. This is precisely what Bowlker ((6), p. 326) did find. With a note of wave length 6·4 in. (frequency 2040 d.v./sec.) and with equal intensities at the two ears he states, "Now three images could always apparently be heard together when facing one of them" (Fact IV).

19. The second limitation, viz. the limit of the time interval between sounds at the two ears which causes the perception of a single source of sound in a certain direction, varies probably with the individual. Though it may depend on physiological processes, it does not depend on them alone. It probably depends on the intensity of the stimulus among physical factors, while psychological factors, especially attention and attitude, have a marked effect.

20. Experimentally[1] I obtained the following results with three observers:

		Time-difference between clicks at ears																				
		0			·87σ			1·75σ			2·6σ			3·5σ			4·4σ			5·2σ		
No. of sounds reported		1	?	2	1	?	2	1	?	2	1	?	2	1	?	2	1	?	2	1	?	2
Observer	P	22	—	—	41	—	—	23	16	9	7	11	20	3	6	19	1	—	4	—	1	13
	M	19	—	1	20	—	—	27	2	5	11	2	5	9	3	17	2	1	3	4	3	10
	W	27	—	—	37	1	—	44	4	—	26	4	9	16	1	12	4	—	4	—	—	1
Total		68	—	1	98	1	—	94	22	14	44	17	34	28	10	48	7	1	11	4	4	24

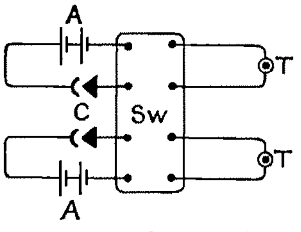

Fig. 2. *A*, 4-volt accumulator; *C*, Keith-Lucas contact breaker; *Sw*, 4-way switch; *T*, telephone.

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[1] The clicks were made by breaking electric circuits as shown on Fig. 2. In each circuit was a telephone without diaphragm. When the circuit was broken a very faint click, of no determinate pitch, was heard. A 'Keith-Lucas' contact breaker was used. By this means the time interval between the clicks could be accurately adjusted.

The above table shows the number of times observers reported they heard one sound (1), were doubtful whether there might not be a second sound (?), or detected two sounds (2), when the time-difference between the clicks at the ears was as indicated at the head of each column.

21. I do not propose to analyse these results in this paper, but it is evident that for observer P the time-difference at which two sounds can be discriminated is between $1{\cdot}75\sigma$ and $2{\cdot}6\sigma$, for the other two observers the limen is higher. From this we may conclude that two clicks, one at either ear, cannot be discriminated when the time-difference between them is of the order of about $1{\cdot}75\sigma$. In this time sound can travel about 57 cm. in air, which is approximately the distance round the head from one ear to the other, so, under natural conditions, a sound of low pitch will always provide the data for a correct judgment of direction. When for experimental purposes a time-difference greater than this is given localization is not accurate; in fact two sources are often located, one on either side (Fact v). Yet even so, given a suitable attitude on the part of the observer, a single binaural sound may be localized.

22. As I have indicated above (p. 148), we would not expect time-difference to produce a correct judgment of direction with notes of high pitch. But for these notes, as Rayleigh (12) pointed out, the head normally produces a marked sound shadow when the sound source is not in the median plane. Rayleigh's calculations are made assuming the head to be a sphere and the intensities are calculated at the ends of a diameter of the sphere. It seems probable that the actual differences of intensity may be much greater than those deduced by such approximations in mathematical calculations. These intensity differences form the basis of our judgments of direction for notes of high pitch (Fact vi), perhaps in the manner outlined below.

23. It is difficult to prove exactly how intensity is translated into loudness. Assuming an 'all or none' law for the nervous impulses, it would seem possible that an increase of loudness is due to a greater number of nerve fibres being brought into operation for more intense than for less intense sounds: provisionally I would accept Hartridge's suggestion (8) that neighbouring fibres of the basilar membrane are excited by loud sounds. If the auditory nerve fibres do not obey the 'all or none' law, it is only necessary to assume a graded impulse as the physiological correlate of graded physical intensity and psychological loudness. If my argument up to this point holds, it is possible that a graded impulse is a large factor; for a more intense sound wave will

cause a greater tension in the cilia, and since *ex hypothesi* the impulses cannot be more frequent for loud than for quiet sounds, they may well be graded.

24. A sound wave falling on one ear will, then, excite the corresponding sensory area of the brain, and, if perceived, the sound will be localized on the side of the ear. All sounds heard by one ear only will appear to come from the same direction if we neglect such factors as our knowledge that the sound comes from a certain street or is produced by a certain horn or is of a certain 'timbre' or loudness which, perhaps unconsciously, indicates the position or direction of the source. Experimentally, if only one ear is stimulated the sound is heard 'at' or 'in' that ear. If the same sound wave falls on the other ear a little later, the intensity being undiminished, the sound will be heard as coming from a certain direction depending on the time-difference between the stimulation of the ears. If the sound is of high pitch, there will, as we have seen, be uncertainty as to its direction, for the time-difference may indicate any of two (or more) directions. If, however, the intensities of the sound waves at the two ears are not equal, another situation arises. The time-difference indicates, with more or less definiteness, that the sound comes from a certain direction, but attention may be distracted from this, for the intenser stimulus is more and more insistent the greater the difference of loudness. There is the binaural (integrated) sound localized as coming from a certain direction *plus* a monaural sound. If the former is well defined, so that the direction from which it appears to come is clear, it will be attended to and the monaural sound may not be perceived. In fact quite considerable differences of intensity may produce no apparent change in the direction from which such a sound will be judged to come. The one effect does not neutralize the other, each being due fundamentally to a different process (Fact VII).

25. With a further increase of intensity the monaural sound will become more and more insistent, till at last two sounds may be heard: a binaural sound in a certain direction, and a monaural sound at the ear which is the more strongly stimulated. With a still greater increase of intensity attention will be wholly absorbed by the monaural sound. If, however, the observer believes that what he hears is due to one sound source only, he may throughout combine the two effects. His judgment, when the intensities at his ears are equal, will be that he hears one sound from a certain direction which is wholly determined by the time-difference; as the intensity to one side is increased the sound will appear to move gradually over to that side. More careful observation, however,

will disclose the fact that two sounds can be discriminated. This is well illustrated in experiments I have recently published (2) (Fact VIII).

26. If, for any reason, the binaural sound is not well defined the monaural sound due to the greater intensity to one side has a much greater effect: attention is much more easily attracted to it. Now, as we have seen, sounds of low pitch will in normal circumstances appear to come from a well-defined direction; so for such sounds intensity-differences at the ears will produce little effect. For sounds of high pitch, since the time-difference gives inconsistent localization, we would expect intensity-differences at the ears to be very effective (Fact IX).

VII. *The Length of the Refractory Period.*

27. We have as yet no information as to the magnitude of the Refractory Period of the auditory nerve fibres. Adrian (personal communication) informs me that the Refractory Period of all the sensory nerves so far studied, viz. Pain, Touch and Temperature, is about 2σ, and he considers it probable that the Refractory Period of the auditory nerve fibres will be of approximately the same magnitude.

28. I am at present engaged on experiments which should throw light on the value of the Refractory Period of the auditory nerve, assuming the hypothesis I have outlined. It is fully in accordance with the hypothesis that the value of the Refractory Period should be as high as 2σ. I shall deal with this question when my experiments have advanced further.

VIII. *Summary.*

29. There is at present no hypothesis which explains the facts of sound localization satisfactorily[1]. Since no theory of hearing can be considered complete unless it affords an explanation of the facts of sound localization, the importance of such a hypothesis to auditory theory in general is very great: it may well be the deciding factor between the many contending theories of hearing.

30. In this paper an attempt is made to show the relation between sound localization phenomena and their underlying physical and physiological factors. Accepting a Resonance theory of hearing, the hypothesis is based on the assumption that nervous impulses are aroused

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[1] I am aware of Boring's recent attempt (6) published since this article was written. I do not, in this paper, criticize his suggestions (which do not explain the facts of sound localization outlined on pp. 143, 144), because I consider it better that my hypothesis should be launched separately.

in the auditory nerve fibres when the cilia of the hair cells of the organ of Corti are in a certain phase of their motion. If this assumption is granted it is shown how the various experimental facts of sound localization, given on pp. 143, 144, can be explained.

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CRITICAL NOTICE

BY F. C. BARTLETT

Aphasia and Kindred Disorders of Speech. By HENRY HEAD. Cambridge University Press. 1926. Vol. I, pp. xvi + 549, Vol. II, pp. xxxiii + 430. £3. 3*s*. 0*d*. net.

I SUPPOSE that every serious psychologist has at some time or another turned with eager anticipation to a study of portions of the vast literature devoted to a discussion of aphasia and allied disorders of speech. For the enormously important part played by language in the development and maintenance of any complex and high grade form of mental life is obvious, and it would seem that an investigation of the disorders of speech could not fail to throw a clear light upon some of the most important problems of the growth and modes of activity of the mind. But, as every serious psychologist had learned to his cost, hitherto there has been singularly little to repay him for his interest in this field of research. Masses of anatomical detail, most of it of the type which Dr Head calls 'dead-house anatomy,' confront him, together with more or less violent disagreement concerning many of these details. He finds diagrams galore, and great luxuriance of *a priori* speculation. His interest probably quickens when he comes across detailed and carefully recorded clinical reports, only to die down once more as he finds that hardly ever is a clear account given of the exact methods by which the clinical observations have been obtained, and practically nowhere is there any sign that different observers are prepared to co-operate in setting up recognized modes of examination. It is perhaps not a very great exaggeration to say that the psychologist emerges from this study with only one definite conclusion. I can well imagine him saying to himself, as he at last shuts up his book and puts aside his articles: "Whenever a physiologist, or a neurologist or any other biological scientist next twits me about the vagaries of psychological terminology I will simply repeat to him a few of the words that are to be found in all this mass of work."

In these two beautifully produced volumes the case is very different. No psychologist can read them without enthusiasm, and every psychologist certainly *ought* to read them. It would of course be absurd to urge that they leave no loose ends, or that they settle finally all of the important questions which they discuss. But here for the first time we have a genuine and consistent psychological method of approach, we have modes of clinical observation which are exactly described and capable of universal application, we have a broad classification based, not upon *a priori* reasoning, but upon a rigorous examination of especially good case material; and we are, as a result of all this, set free entirely from that doctrine of precise anatomical localization which has worked disastrously in earlier discussions, and put on the way towards a more thorough understanding of the significance of all forms of 'symbolic formulation' than has hitherto been possible.

There is material in these books which should lead to prolonged further experimentation and discussion. In a brief review all that I can attempt is to bring out a few of the points which make Dr Head's treatment of peculiar interest to the psychological reader.

A short preface is followed by a hundred and forty pages of historical discussion in which the evolution of our knowledge of cerebral localization is brought under review. To me this seems a most brilliant piece of work. It is witty, concise, lucid, and, within the limits which Dr Head sets for himself, scrupulously fair. "I selected certain epochs and men," he says, "as representing some definite aspect of thought. I have made no attempt to give a consecutive and coherent account of the ideas held at any one time concerning such groups of symptoms as for instance alexia, agraphia and the like. Owing to this personal method of presenting the historical facts I have been unable to notice or give due weight to the admirable contributions of many observers whose conceptions tallied with those held by more prominent protagonists.

Moreover, since I hold definite views of my own this historical introduction cannot be an unprejudiced account...." In actual fact Dr Head has made a good road through a peculiarly baffling forest, and it is to be hoped that in future anybody who wants to explore the bypaths of the forest will first pass along this road.

Head's statement of the results of his historical survey is vitally important for an understanding of the point of view of his own contribution:

> "Most of the work on aphasia has been vitiated by a subtle and deep-seated error. It was almost universally assumed that the phenomena revealed by analysis could be treated as elements, which were independent and had entered into combination....All conscious processes were reduced to sensory or motor presentations and laws of association. Mental activities were not considered from the dynamic point of view, but as a static synthesis of constituent factors.
>
> It was assumed that for every mental element there was a neural element, either identical or in exact correspondence with it. Such a conception made it possible to

postulate regionally distinct centres for diverse aspects of speech. Any one of these could be destroyed independently and the result would be manifested in terms of one of the constituent factors, motor, auditory or visual. From such *a priori* principles it was possible to deduce in turn the defects of function, which must follow destruction of each centre or internuncial path, and the form assumed by the manifestations of organic disease could be confidently explained by an exact study of the site of the lesion.

But these attempts to explain a vital process in terms of dead-house anatomy were bound to fail."

We must return to the position advocated years ago by Hughlings Jackson. Loss of speech is first and foremost derangement of a set of *psychical* reactions. No doubt cerebral injury is involved, but "to locate the damage which destroys speech and to locate speech are two different things." Meanwhile the very first requirement is to find out exactly what disturbance of psychical functions *is* brought about by the 'loss of speech' which marks aphasia in all its varied forms.

This leads Dr Head to his Part II in which he describes in detail his methods of clinical examination, and their results so far as his own observation goes. The methods are a great advance upon anything that has preceded them. They consist in the main of a series of definite tests which any new observer can perfectly easily take over and apply for himself. It is obviously a matter of great importance that in forming any views about the significance of clinical observations we should know exactly how such observations have been obtained. For records of this sort are bound to be highly selective and it is well nigh impossible to estimate their value unless the ways in which they were secured are so exactly described that they can, if opportunity offers, be applied again by the student. There is another reason which makes a precise account of methods of examination particularly important in the study of aphasia. Dr Head often insists, and no doubt rightly, that to assume that "the diverse defects of speech which constitute aphasia reveal the elements out of which the use of language is composed" is a fatal error. The aphasic defect produces, not historical fragments of the content of a complex mental process, but literally a different set of reactions. Though the study of aphasia however does not give us historical *elements*, it certainly may yield reactions which possess very great historical interest. Head's records suggest again and again that his subjects were often reverting to relatively early ways of dealing with the situations presented, ways characteristic of a mental life which has not yet acquired the capacity to deal freely with word symbols. There is of course no absolute reversion. The methods of the aphasic patient remain the methods of an aphasic and do not literally become those of a mental life which has not yet acquired the normal speech reactions. But there is clearly the greatest need here for very careful comparative study both with children and with normal people, and Head's methods make this comparative work possible.

Having described his methods Dr Head passes on to consider the results of their application. The evidence is marshalled with great skill. It leads to the conclusion that the functions which suffer in aphasia and kindred speech disorders are those of 'symbolic formulation and expression.' I confess that for a considerable time I thought this way of characterizing the defects less accurate and less expressive than Hughlings Jackson's treatment of them as defects in the capacity to 'propositionize.' For it is clear enough that many symbols can still be used by the aphasic person. What Head calls the process of 'matching,' where one sensory pattern stands for (or symbolizes, in Head's sense of the word) another sensory pattern—as where an object which has hitherto been seen only is identified after tactual exploration in the absence of vision—remains practically unaffected. Concrete sensory imagery is often undisturbed and even very vivid. I think it probable that Hughlings Jackson wanted to indicate that it is not symbols as such that suffer, but the power of using and understanding them in their capacity as conveying and expressing certain relatively abstract meanings. But Head's phrase 'symbolic formulation and expression' meets this point sufficiently, as I now believe, provided enough emphasis is laid upon the '*formulation*,' and it has the further advantage of not for one moment suggesting that the aphasic disorder is one that is expressed in defective *language* reactions, and in those alone. To recognize this seems to me a great step forward and to be mainly due to the adoption of a genuinely psychological mode of approach. Words are undoubtedly the most effective of all the instruments we have for dealing with situations at a distance, without being distracted unduly by purely particular details. But they are not the only means, and Head shows clearly that all 'symbolic formulations' which perform the same kind of function that words perform suffer when the propositional functions of speech suffer.

Dr Head has already expounded in this *Journal* (Vol. XI, pp. 179–193) his proposed fourfold classification of disturbance of function in aphasia into verbal defects, syntactical defects, nominal defects and semantic defects. The basis of the classification is of course far more fully presented in these volumes than was possible in an isolated article.

Such classification is valuable because it directs attention upon points which are of outstanding importance for discussion. Nobody could be more insistent than Dr Head himself that all such classifications must be made to serve the purposes of systematic treatment, but never for a moment allowed to dictate theories. It is however almost inevitable that theoretical disputes should arise about such classifications. Somebody, for example, will be almost sure to point out that the verbal and nominal forms seem to be word function defects, while the syntactic and semantic forms, in so far as they are specifically language forms at all, seem to be proposition function defects. And thus the whole question of word units and proposition units in language may perhaps come on the stage again.

The third part of Vol. I deals with the question of the localization of function, and with the anatomical site of the lesion in some traumatic cases of aphasia. Broadly Head found that his four typical disorders of speech function presented lesions in differing areas of a single cerebral hemisphere, the deeper lesions producing more clearly marked and more permanent defects. He is however wholly opposed to the doctrine of 'centres' for the various specific processes, reading, writing, speaking and so on, involved in the normal use of language.

Perhaps it is to Vol. I, Part IV that the psychologist will turn with special delight. For most psychologists, it is to be feared, still love theories best of all, and here Dr Head presents his general view of the ways in which life and mind have grown and are organized. There is indeed no airy and groundless speculation. His discussion is based upon a life work of patient and brilliant scientific observation and experiment. These three brief chapters on Vigilance[1], Cerebral Localization, Language and Thinking, and the final Conclusions and Summary, are in fact the theoretical presentation of the whole of Dr Head's neurological and psychological investigations and have as their base all the mass of facts recorded in his Collected Papers, in these two large volumes, and no doubt many more that have escaped record.

I cannot attempt to deal here with Head's remarkable and for the most part convincing constructive effort, particularly as I hope to discuss his theories in greater detail elsewhere. He shows how behind every, even the most apparently simple, human reaction lie a mass of integrations of a physiological and often of a psychological nature as a result of which the behaviour of the moment has to be regarded as *directed* or adapted to some end which is in process of achievement by the organism.

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[1] See also this *Journal*, XIV, 126–147.

Integration and adaptation are present at all levels of response but are more complex, complete and evident at high levels. They are then dependent in the main upon the vital condition of the central nervous system, a function to which Head assigns the name 'vigilance.' 'Vigilance' may be disturbed by local structural changes in the central nervous system, by toxic influences, by fatigue, by certain states of sleeping probably, and in other ways. All these may upset, and perhaps prevent, the "fulfilment of some specific form of behaviour, and the reaction which follows expresses the response of the organism as a whole to the new situation." Throughout all the development of life the controlling function is to be understood in terms of 'vigilance,' while the immediate actual control is effected by means of relatively lately acquired specific processes. Thus the higher grade reflexes control those of lower grade, and in absolutely the same way conscious processes control higher grade reflexes. Whether certain specific processes are going to be in control in any given case is however a question of 'vigilance.' Thus the dualism which we commonly make of bodily and conscious processes, and which is still at the basis of most accepted psychology and physiology, is all wrong and raises entirely spurious problems. We never do have bodily processes complete in themselves on the one side and mental processes complete in themselves on the other side, and then the two sets somehow brought into relation by interactionism or parallelism or something of that kind. We have instead the gradual building up of more and more complex integrations and more and more complicated adaptations. Within these the controlling function is always exercised by the late acquired physiological or psychological processes. The control is exercised in the same general way and through the same general characters always. "There is no more difficulty in understanding how an act of consciousness can affect a physiological process, than to comprehend how one reflex can control and modify another of a lower order." But whether the control in a given case is to be by a reflex or by an act of consciousness, and by what reflexes and what acts of consciousness, are in part a question of the stimuli of the moment in their relation to various observable local states of the organism, and in greater part still of the state of the vital and ever active function termed 'vigilance.'

All of this is worked out, it is needless to say, not in general terms merely but with reference to the specific problems of aphasia and kindred disorders of speech. The treatment is so packed and condensed already that any attempt at further condensation, such as the one which I have made here, must be accounted lucky if it escapes the effect of giving a mere parody.

With the general ordering and march of Dr Head's ways of thought I find myself in complete and enthusiastic agreement. I believe that psychology at any rate would become more genuinely scientific, more fruitful, more living if they were generally adopted. But at various points I still find serious difficulty. Most of these I must leave for the present but I will mention one or two.

In the development of his views about 'vigilance' Head makes considerable use of his doctrines of 'schematism,' taking them over exactly as they were developed in his earlier papers. Now as to the necessity and value of the theory of schemata there can be no doubt whatever. In some way or another, under some terminology or another, notions of this sort have found a place in practically all live psychological construction. But I cannot escape from the conviction that the assumptions involved in the doctrine must be far more fully worked out. The selective function of building the schemata is one thing; the direct function of a schema itself in determining a given concrete response is another thing, and the functions by virtue of which integrations of schemata are effected is perhaps a third thing still. These cannot all, I think, be satisfactorily referred to an underlying general 'vigilance.' Schemata may be built up in many different ways and the differences may turn out to be psychologically far more important than the fact that, however different they are, they can all be called 'schemata,' and all be treated as having the same general characteristics and effects.

Again I doubt very much whether the theory of 'vigilance' affords anything like as neat a solution of the body-mind difficulty as appears. Dr Head writes definitely of conscious processes as "expressions of physiological vigilance." It is hard to see exactly what this means. "Consciousness," he says, "forms one of the factors in *initiating* (my italics), regulating and adapting vital reactions." This, I think, implies that once the conscious level is reached reactions not hitherto possible now become actual, and that something more is effected than the mere extension of the range of integrations, and the more obvious appearance of purposiveness of response. In other words consciousness does something different, and does not merely carry further the old lines of development. If this is so it may turn out that the long standing problems concerning mind and body break out again within that very 'vigilance' which is supposed to reconcile, or even to obliterate them all.

No doubt many answers could be given to these and other difficulties. I believe that the true answers are indicated but not fully developed in various detailed parts of Dr Head's own discussions, and that, building upon these discussions, they will eventually be found.

Volume II gives full clinical reports of illustrative cases. These are delightful reading and must be carefully studied by everybody who desires to be in a position to appreciate the full force of the views put forward in the first volume.

For the psychologist these are two of the most important books that have appeared for many years. They are scientifically and aesthetically satisfying. It is risky to prophesy; but I believe that these books will have a permanently important place, not only in the study of aphasia but in the history of the shaping of psychology into a science. The test methods may be developed, extended, diversified, their complexity more fully recognized. The lines of classification of defects may perhaps be altered again, and the terminology proposed will in all probability suffer further change. The general theory will undoubtedly be developed as it is worked out with greater detail. But the method of approach will remain, the clinical observations will remain, in my opinion the general view point will remain of abiding interest and of lasting importance. And, as with all genuinely valuable achievement, Dr Head's life work, and particularly this crowning effort, will be a most powerful stimulus to further research.

PUBLICATIONS RECENTLY RECEIVED

G. Stanley Hall: A Biography of a Mind. By LORINE PRUETTE. New York and London: D. Appleton & Co. 1926. pp. xi + 267. $2.50.

This is an odd but rather discriminating biography. It makes no pretence at telling an orderly story but selects freely with a somewhat disconcerting disregard for chronology. It mixes a certain amount of ironical insight with no small quantity of hero worship. It is often frankly critical, more often still anxiously appreciative. It tries to be "good writing" nearly all the time; and succeeds some of the time. Stanley Hall remains, as perhaps he was bound to remain, rather misty at the edges, rather incoherent, hard to see through the masses of words he constantly seems to have thrown out, and on the whole not very interesting at a distance. He would probably have liked this book, and he would probably have been right. Take it on the whole it is a good effort, though spontaneity seems somehow to have been crushed out of a lot of it.

The Psychology of the Poet Shelley. By EDWARD CARPENTER and GEORGE BARNEFIELD. London: George Allen & Unwin, Ltd. 1925. pp. 127. 4*s.* 6*d.* net.

Nearly half of this book consists of a most interesting essay by Edward Carpenter dealing mainly with the significance and direction of what he calls the "love interest" in Shelley's life. Mr Barnefield is a whole hearted adherent of the view that sex abnormality (bisexuality in this case) accounts in the main for Shelley's genius, and treats his best work as a sublimated expression of "the repressed homosexual component of his nature." He pursues his theories with much vigour and no little skill, and has probably, like a good many enthusiasts, captured a part of the truth.

The Subconscious Self, and its Relation to Education and Health. By Dr LOUIS WALDSTEIN. New Edition, with a biographical sketch by Sir CHARLES WALSTON. London: Charles Scribner's Sons. 1926. pp. lxvii + 171. 7*s.* 6*d.*

Dr Waldstein's brief essays dealing with the influence upon conduct and mental life generally of subconscious factors were first published in 1897. They are undoubtedly of great interest, alike for the facts they record as for their theories, though they are not at all fully developed or systematic. To this edition Sir Charles Walston contributes an extremely graceful and well written biographical sketch. Tributes to Dr Waldstein's scientific devotion and skill are paid by a number of well known authorities, among the rest by the late Sir Clifford Allbutt and Professor G. F. Nuttall.

Complacency: The Foundation of Modern Behaviour. By Dr R. B. RAUP. Introduction by Professor W. H. KILPATRICK. New York: The Macmillan Co. 1925. pp. xii + 201. 8*s.* 6*d.* net.

The title of this book turns out not to mean what at first it may be taken to mean. On p. 2 there is a footnote: "The word 'complacency' is here used in its etymological sense more than in accordance with its popular meaning of smugness or self-satisfiedness." The etymological sense apparently is that complacency means a "satisfactory working relationship" between anything and something else, such relationship remaining "relatively unconscious" when a human being is on one side of it. This, which has four phases, (*a*) the formative phase, (*b*) the quiescent phase, (*c*) the disturbance phase, and (*d*) the resumption phase, is the explanation of everything that ever happens. Here it is discussed in relation to physics, chemistry, the physiology of the nervous system, psychopathology, the growth of habit, and philosophy. The author is persuasive and undeniably interesting but less detailed than the scientific reader will demand.

Neue Psychologische Studien: Arbeiten zur Entwicklungs-Psychologie. Herausgeber FELIX KRUEGER. München: C. H. Beck'sche Verlagsbuchhandlung. Erster Band, *Komplexqualitäten, Gestalten und Gefühle.* 1926. pp. 692. Geh. M. 35. Zweiter Band, *Licht und Farbe.* 1926. pp. 442. Geh. M. 32.

No adequate review of the enterprise of which these are the first two volumes can be given here. But unquestionably the publications are of great importance to the student of contemporary German psychology and philosophy. It is indeed probable that every English reader will frequently sigh for a little more precision and much less display of language. He will, however, find a great wealth of interesting and significant material included in these large books, much of which will more than compensate him for the time which it may take him to read them.

In the first volume, after a foreword, the general editor discusses the character of "psychical wholeness"; Dr Sander writes on "spatial rhythm," and the psychological significance of optical illusions: Dr Gunther Ipsen considers form perception (*Gestaltsauffassung*) mainly in relation to the optical illusion known as Sander's parallelogram, and "the theory of knowing" chiefly in reference to the form (*Gestalt*) and significance (*Sinn*) of meaningless words; Dr Johannes Hermann deals with the general nature of smell experiences; Dr Erhard Lenk writes on the visual perception of regularly constructed geometrical forms, and Dr Johannes Rudert concludes the whole with an article on the functional asymmetry of the cerebral hemispheres.

Professor August Kirschmann participates in the editorship of the second volume, which contains articles on number systems by Dr Emil Bergfeld; on light and colour-contrast by Dr Thorlief Schjelderup-Ebbe; on colour terminology by Dr Kirschmann; on the functional relationship between stimulus and sensation in regard to colour saturation by Dr Friedrich Donath; on colour memory and its relation to studio and open air painting, by Dr Fritz Ehrler; on the position of colour qualities in the colour circle and their complementary relationships by Professor Friedrich Weissenborn; on the black-white problem by Dr Kurt Fiedler; and on the colours of the inverted spectrum and the significance of this for optical science, by Dr Kirschmann. This volume contains a number of very well produced colour diagrams.

Intelligence and Immigration. By DR CLIFFORD KIRKPATRICK. Baltimore: The William Wilkins Company (London: Baillière, Tindall & Cox). 1926. pp. xiii + 123. 18*s*. net.

Although this most careful investigation of the intelligence of immigrant groups directly concerns only the United States of America, it is well worthy of study by students of social policy in any other modern nation. The author presents first a sober discussion of principles and definitions; next a comprehensive survey of the work of other investigators; after that a concise account of his own tests and their results, and finally an illuminating chapter dealing with the general implications of the main results, and giving suggestions for a future immigration policy. The whole study may be warmly recommended. In general it presents a strong case for selective immigration, the basis of the selection being largely psychological.

An Outline of Abnormal Psychology. By J. W. BRIDGES. Third Edition, Revised. Columbus, Ohio: R. G. Adams & Co. 1925. pp. 236.

The third edition of this well known and useful book of definitions and references contains several new sections, particularly in relation to instinct and emotion, intelligence, and personality. The general effect of these additions and of various other changes is to bring the treatment up to date. The book remains exactly what it claims to be, a list of abnormal mental phenomena, a classification of their main symptoms, and a brief summary of the most widely recognized explanatory theories. Its main value is to serve as a "refresher" for a student who has covered a wider course of lectures or of reading.

Psychotherapy. By Dr E. W. TAYLOR. Harvard Health Talks, 14. Cambridge, Mass.: Harvard University Press (London: Humphrey Milford). 1926. pp. 53. 4*s.* 6*d.* net.

In this very lucid and interesting lecture Dr Taylor traces the development of psychotherapy from early times to the present. He shows the various underlying notions which have marked different stages of mental healing, and gives a brief but very clear characterization of the "conflict" hypothesis which is now current. "The time has certainly come," concludes the writer, "when our knowledge, imperfect as it is, is sufficient to accept the broader implications of treatment which a rational psychotherapy permits. I emphasize the word 'rational,' since we have finally attained knowledge in this difficult but most important field of research, which every well-trained physician must in some degree master, if, as he should, he sets himself the task of treating disease by all legitimate methods."

Psycho-Analysis for Normal People. By GERALDINE COSTER. Oxford University Press. London: Humphrey Milford. 1926. pp. 232. 2*s.* 6*d.* net.

This is "an effort to set forth in the simplest possible way the main principles of analytical psychology in its application not to the insane, perverted or abnormal, but to the ordinary people whom we meet every day." It is meant to be "entertaining enough and brief enough for nurses to read in their rare moments of leisure," for school teachers, and in general for people who cannot spend either much time or much money in studying the work of certain contemporary psychologists. The essay may safely be recommended, for it is both clear and well informed. It will indeed leave a critical reader with a mass of difficult problems, but that is inevitable.

Psyche: The Cult of Souls and Belief in Immortality among the Greeks. By EDWIN ROHDE. London: Kegan Paul, Trench, Trübner & Co., Ltd. International Library of Psychology, Philosophy and Scientific Method. 1925. pp. xvi + 626. 25*s.* net.

It was a fine enterprise on the part of the publishers of this well known series to include a translation of Rohde's monumental work. The rendering, which is due to Mr W. B. Hillis, is an extremely good one. It is from the eighth German edition and the translator has fortunately endeavoured to systematize footnote references, adding, where possible, references to English translations. All psychological students who are interested in the development of religion, or conversely in religious regressions, will find in this volume a mine of important and suggestive material.

Comparative Philosophy. By PAUL MASSON-OURSEL, with an Introduction by Dr F. G. CROOKSHANK. London: Kegan Paul, Trench, Trübner & Co., Ltd. International Library of Psychology, Philosophy and Scientific Method. 1926. pp. 212. 10*s.* 6*d.* net.

This book covers a vast field. After a general introduction the author discusses what he calls the "positive notions" of analogy and environment, and urges that the comparative study of the history of thought provides the "true critique of the mind." Then comes a lengthy table of dates giving "a comparative chronology of philosophy." Part II, translated by V. C. C. Collum, deals with comparative chronology, comparative logic, comparative metaphysics and comparative psychology. The conclusion urges the pedagogical importance of the comparative method in philosophy. The only chapter of direct interest to readers of this *Journal* is the one on comparative psychology. This, though scrappy, is undoubtedly well worth reading for it helps to show how much fruitful variation is possible in regard to certain fundamental psychological concepts which Western psychology in particular is apt to regard as fixed in its own current forms.

NOTES ON RECENT PERIODICALS

Zeitschrift für Psychologie.

Bd. 97, Hefte 1 und 2, 1924.

Experimentelle Untersuchungen uber Umstillbarkeit. (Dr Maria Zillig.)

The ability to change rapidly from one subject to another is by no means correlated with general intelligence or ability.

Über Momentadaptation. (Fritz Noldt.)

Experiments were carried out on the following aspects of the adaptation of the eye to light and darkness: Variation in duration and intensity of stimulation: examination of the retina during stimulation: influence of movement, (*a*) of the eye, (*b*) of the stimulus (the adaptation was found to be more rapid in the latter case): enlargement of the pupil: and the effect upon the after-image.

Psychologische Untersuchungen an nichtdomestizierten Nagetieren, namentlich der Hausmaus. (Dr des H. Roth.)

A record of the educability of wild mice, in an investigation to ascertain their speed in learning to distinguish between right and left; their ability to distinguish colours, and tastes, as well as an investigation of their kinaesthetic memory.

Grundfragen der Akustik und Tonpsychologie. (E. R. Jaensch.)

IV. E. R. Jaensch and Georg Rothe. *Die psychologische Akustik der Sprachlaute in ihrer Beziehung zu Lagestellungen der Wissenschaften von der Sprache.*

These authors describe their experiments, based upon the synthetic investigation of vocalization of Helmholtz, in connection with its significance for general science as well as that of speech; the relationship between vocalization and clang and the results of vocalization acoustics upon the developmental history of speech, song and music.

Die Auffassung der psychischen Structuren vom Standpunkt der Assoziationspsychologie. (Th. Ziehen.)

1. Whether and how, according to the rules of association, the phenomena of ideational stimulation are to be explained by the partial representation contained in these complexes.
2. Which special intellectual combinations of these phenomena express ideational stimulation, for example, partial representation, either simultaneous or successive.

There can be no doubt, concludes the writer, after examining the above problems, that their final solution will only be achieved through systematic, experimental investigation, in which abundant consideration must be given to the introspection of the subjects.

Zeitschrift für Psychologie.

Bd. 97, Hefte 5 und 6, 1924.

Über den Einfluss des Weissgehaltes des Infeldes und des Umfeldes auf die dem Infelde entsprechenden Erregungen. (G. E. Müller.)

Discusses the author's own theory of colour vision as well as that of Helmholtz in relation to Induction and Simultaneous and Successive Contrast in connection with (1) the anti-chromatic influences of white light, and its combination with pro-chromatic influences, (2) the determination of the colour limen and the distinction of colour generally from brightness of disc or background. The results of Révisz's experiments by Gelb and Granit are considered unsatisfactory, especially upon the problem of heightened colour limen in cases of a small coloured figure upon a background.

Bibliographie der deutschen Literatur des Jahres 1924 über Psychologie, ihre Hilfeswissenschaften und Grenzgebeite, zusammengestellt von Friedrich Sander.

A valuable index of journals and books.

Zeitschrift für Psychologie.

Bd. 98, Hefte 1 und 2, 1925.

Über den Einfluss des Weissgehaltes des Infeldes und des Umfeldes auf die dem Infelde entsprechenden Erregungen. (G. E. Muller.)

(*Continuation of paper in Vol.* 97, *No.* 6.)

Continues investigation upon the influence of induction of a coloured field upon the recognition of its colour. The Colour Limen for a colourless disc is *least* in consequence of the influence of induction upon the white mass of this contrast disc, when it is surrounded with a background of equal brightness. The determination of the Limen and the Clearness of Colour Contrasts upon the brightness of the disc and background are discussed, using four methods of proof: (1) Objective imitation; (2) Compensation; (3) Comparison of different contrasting colours; (4) that of Katona, the method of the Constant Limen; and also the Differential Limen in the case of colourless discs and backgrounds.

Abstraktion und Widererkennung. (Geza Révisz.)

Révisz considers to what extent the lower apes are capable of mental processes of abstraction and recognition, on the basis of a series of experiments upon children, adults, and monkeys. Results showed that children between 3½–6 showed a greater appreciation for colour than form, children over 6 for form, adults again based their selection upon colour and the monkeys recognized form before colour. The writer states that the colour preference of young children is a natural one, which the older children lose because school influence accentuates form, whereas the adult is able to return to the natural selection of childhood.

Das willkürliche Anhalten des Herzschlags. (Hans Henning.)

Upon the subject of the voluntary arrest of cardiac action by mediums, fakirs and hysterics, Henning asserts that none of these can endure medical investigation. Several ingenious exposures of "tricks" are given.

Wünschelrutengänger ohne Wünschelrute. (Hans Henning.)

An experiment carried out in the Waltherplatz of Bozen, by several dowsers, who, by walking over the ground with metal or wooden divining rods, were able to diagnose the presence of water beneath. After their findings had been recorded, D. Heineman made the same discoveries with his hands only, and stated that he was always able to diagnose the presence of water, and by the twitching of his fingers determine the depth at which it was located and any mineral properties that might be contained in it. No explanation of this phenomenon is offered.

Einige Sinnestäuschungen und ihre Ursachen. (P. Plassmann.)

Optical illusions connected with the sun are explained by the writer of this paper to be due mainly to the fact that they arise when we see the sun under unaccustomed circumstances, or when the usual atmospheric influences are excluded.

Studien uber Strukturgesetze. IV. Über Mikromelodik und Mikroharmonik. (Heinz Werner.)

In opposition to Stumpf and Brentano, who consider the question of micro- and macro-melody and harmony to be purposeless, this author believes that such a study would be of significance in the three spheres of musical science, psychology of sound and finally of developmental psychology; because by this means, many harmonics and overtones would be brought into prominence which now entirely escape notice in the general use of the normal system, based upon the octave and its recognized component parts.

Zeitschrift für Psychologie.

Bd. 98, Hefte 3 und 4, 1925.

Über die Vorstellungswelt der Jugendlichen und den Aufbau des intellectuellen Lebens. VI: *Über psychische Selektion.* (E. R. Jaensch.)

An experimental investigation of the relation of the "estimating" to the "eidetic" side of consciousness, with observations upon the discussion raised by psycho-analysis, including supplementary remarks upon "understanding" and the relationship of psychology to "phenomenology" as well as to other contemporary trends of philosophic research.

Versuche über die Entwicklung des visuellen Gedächtnisses bei Schulern. (Wilhelm Bergemann.)

A review of previous investigations of this problem shows that former research has determined that different kinds of memory are developed at different ages, as well as a varied speed of forgetting. Recent research, however, upon the development of eidetic capacity, appears to offer some contradiction to earlier opinions. The Reconstruction Method, supported by G. E. Muller, is founded upon a theory of association of ideas. The present experiments tested the reconstructive abilities of the children and incidentally their capacity for eidetic representation. It was found that the children who worked with the greatest rapidity were usually the most correct, but that those whose test coëfficient was the highest, reversed this grading in daily school work and *vice versa.*

Zur Psychologie der Tondistanz. (Otto Abraham und E. M. von Hornbostel.)

A description of experiments carried out by these writers from 1905–14. The research is primarily limited to two tones next to one another in the scale, that is, the interval of one whole tone. That the quality of the interval depends upon the difference in vibrations is universally accepted. Here three hypotheses are discussed: (1) That the size of the interval expresses the difference of number of vibrations. (2) The size of interval expresses the vibration relation. (3) The interval does not express the geometrical relation but exceeds this in the higher notes.

Results of research proved that two tones struck together allow us to distinguish two characteristics: (1) distance or clang-volume, which is quantitative inasmuch as it can increase, (2) is qualitative and bestows upon the interval its typical "combination" tones.

Zeitschrift für Psychologie.

Bd. 98, Hefte 5 und 6, 1926.

Enkapsis. Ein Beitrag zur Structurphilosophie. (Karl Groos.)

The title which introduces this contribution was taken from a pioneer work of the anatomist Heidenhain, upon *Formen und Krafte in der lebendigen Natur,* in connection with the arrangement of numerous histological systems combined in muscular structure; "they are placed one above another, encased in one another, *encapsulated.*" Upon this analogy, Groos works out a metaphysical system to a universal philosophy, which he believes to solve the problem of the individual as member of a social system.

Über Ganz- und Teillernverfahren bei vorgeschriebenen Rezitieren. (Willi Meyer.)

An analysis of results of experiments upon learning, either of nonsense syllables, poetry or prose, and whether it may be more easily achieved as "wholes" or "parts." The findings were that for rapid learning in the case of adults and children, part repetition was the most satisfactory, but for retention in memory, "whole" methods proved the more lasting.

Transformationserscheinungen bei Gewichtshebungen. (Hermann Fischel.)

Experiments upon so-called transformation phenomena connected with auditory and visual stimuli, as well as former research upon those who had lost part of a limb through amputation, lead to this investigation upon the transformation phenomena, which appear when lifting weights. A series of experiments was devised, comprising those testing impressions gained from lifting weights with a hand that was already weighted, which acted as a brake upon the fingers, or with bound forearm: upon comparison of judgment by entirely different groups of muscles, such as hand and teeth; hand and forehead; hand and foot; experiments with lessening the actual weight of the lifting arm; with children and the blind.

The group with blind persons proved that the phenomena are not dependent upon optical expectation.

Die Wahrnehmungszeit. (Dr F. F. Hazelhoff und Heleen Wiersma.)

A. Wahrnehmungszeit und Reaktionzeit. 3rd article, conclusion.

To obtain an idea of perceptual time, it is customary to take the reaction time of the individual. Contrary to expectations, there are persons with quick perception, who nevertheless react slowly, so that individually there seems little connection between speed in perception and speed in reaction. It is suggested that other experiments should be made with this system, especially upon the influence of the colour of light upon the speed of perception, whether there is any correlation in visual or auditory types, and if such experiments could be of service for the diagnosis of persons inclined to mania or severe preoccupation.

Zeitschrift für Psychologie.

Bd. 99, Hefte 1 und 2, 1926.

Die Erblichkeit der Musikalität. (Hans Koch und Fridtjof Myoen.)

This paper carries further the research by Haeker and Ziehen upon Heredity and Development of Musical Talent. In both investigations material was collected by means of a *questionnaire* distributed among a wide circle of persons of various professions and both sexes. Over three hundred were afterwards submitted to analysis and tables of the results and statistics are given. These tables however are somewhat confusing at times, and the evidence of some appears to contradict that of others.

Über die kleinsten vibratorisch wahrnehmbaren Schwingungen. (David Katz und Fritz Noldt.)

An investigation which includes research made upon tactile perception of the vibrations of amplitude taken by the fingers of the two authors. The sensitivity of Noldt was found to exceed that of Katz. It is believed that vibrations of amplitude less than 1/100 m. might be perceived by tactile methods, but it is difficult to find subjects of sufficiently acute sensitivity.

Zeitschrift für Psychologie.

Bd. 99, Hefte 3 und 4, 1926.

Zur Psychologie der Gegenwart, Eine Anmerkung zu Koffkas Darstellung. (Otto Seltz.)

Selz asserts that the chief theories represented in the Psychology of Shape had previously been put forward by himself. That is to say, *the theory of the determination of constellations,* and the *three principal points of voluntary mental processes,* (*a*) the theory of schematic anticipation, (*b*) the laws for the enlargement of constellations, and (*c*) the theory of the mental processes such as the constitution of the single discharge.

Verhalten eines Kindes bei Behinderung eines Armes. (David und Rosa Katz.)

An account of observations upon a child of three years and three months, who, through an injury to the left clavicle, was obliged to have his left arm confined to his side with rubber strapping for ten days. It was remarked that this child made no attempt to free the arm, nor to ask for the bandage to be removed; that it in no way hindered his play, and that with great rapidity he adopted new, compensatory methods of accomplishing familiar activities, which usually necessitated the use of both arms.

Bunte Farben in bunter Beleuchtung. (Hellmut Bocksch und Stephen Krauss.)

A discussion of the various theories of perception of colour, and the influence of coloured lighting upon this perception.

Die Helligkeit schraffierter Fachen. (A. Prandtl.)

We find here an interesting investigation of the psychological effect produced by the use of "hatched" surfaces in graphic art, which by careful arrangement of alternate black and white lines, or dots, give the impression of light and shade, relief and perspective. The cause of this, the author states, must be the production of "feeling," or a state of consciousness, since the actual visual perception cannot give the corresponding sense of colour which is really derived.

Vergleichende Untersuchungen zu Psychologie der optischen Wahrnehmungsvorgänge. (Oswald Kroh.)

1. *Experimentelle Untersuchungen zum Problem der Sehgrossenkonstanz beim Haushuhn.* (Walther Götz.)

Fowls, when fed, will first select all the larger grains, or morsels of food, which have been thrown down to them, and only when these have been finished, will they seek those further off, and at the last pick up the smaller ones. It is held upon evidence provided by a series of experiments upon these birds, that their behaviour is determined by optical phenomena, and that the picture presented upon the retina of the hen's eye, shows the grains at some little distance away larger than those in its immediate neighbourhood.

VOLUME XVII JANUARY, 1927 PART 3

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PLEASURE, PAIN AND CONATION

BY WILLIAM McDOUGALL.
(From Harvard University, U.S.A.)

THROUGHOUT the nineteenth century psychology was largely dominated by the theory of psychological hedonism, the theory that in some sense pleasure and pain are the source or grounds or cause of all mental activity, of all conation. In the psychology of the classical associationism of the utilitarian school, hedonism (I omit henceforth the qualifying 'psychological') and associative reproduction were the two governing principles of all mental process. But many other writers of great authority who gave to association a lesser rôle in our mental life were essentially hedonists, in that they regarded conation as determined or evoked by pleasure or pain; Lotze, Bain, Wundt, James Ward may be mentioned. Hedonism has perhaps relatively fewer exponents at the present day. A large number of psychologists have adopted a strictly mechanistic view of mental life and behaviour, a view which finds no place for pleasure and pain, or at least assigns to them no rôle. Others, like Prof. E. Thorndike, adopting a mechanistic interpretation of behaviour, deny all purposive activity, all goal-seeking, all striving towards ends; yet continue to assign to pleasure and pain the quasi-mechanical functions of 'stamping in' or 'stamping out' associations or brain-links fortuitously formed.

Among many of those who are not strict mechanists, who recognize conation, striving towards ends, goal-seeking, conative impulse, desire and volition as other than mechanical sequences, hedonism continues to be accepted; and by many it is combined in various ways and degrees with the hormic theory.

By the hormic theory I mean the view (which comes down to us from Aristotle) that men and animals are natively endowed with dispositions or tendencies to strive towards certain goals proper to the species, such tendencies being brought into play and guided by cognition of appropriate objects.

Very few authors have expounded this theory in its pure form, or have attempted a purely hormic psychology of action. Schopenhauer and von Hartmann and, I think, Ribot are among these few. One can point to but very few contemporary authors who explicitly maintain this purely hormic psychology[1]. By other authors who recognize the hormic principle, it is commonly combined with hedonism. Some actions are interpreted hormically, others hedonically. Ward, Stout, Freud and F. L. Wells come to mind as prominent instances. In a recent article[2] I have pointed out how Freud has inclined in his later writings to give primacy to the hormic principle, and to make less of his 'pleasure principle.' Freud's psychology seems to me to show clearly that this combination of the two principles, the attempt to run them in double harness, results in confusion and error, both theoretical and practical.

My purpose in this paper is to try to show that a purely hormic psychology is tenable. I do not dwell upon the difficulties of pure hedonism. They have frequently been pointed out and never overcome. I am concerned rather to consider the more obvious of the difficulties in the way of a purely hormic view. Before considering these difficulties let me briefly point out the superiority of the hormic principle to the hedonic in face of the major types of activity.

When we act with purpose in the fullest sense, when we set before ourselves a goal and with clear consciousness of it as the object of desire, choose means towards it and pursue those means, progress towards our goal brings pleasure or satisfaction no matter how hard the labour; and attainment brings satisfaction which is the greater, the keener the desire that moved us and the more strenous the efforts we have made. The goal in itself may be of little or no intrinsic value; it may be the barren and shapeless top of a mountain enshrouded in thick mist. But we have desired it, have striven for it, have attained it, and pleasure or satisfaction results. On the other hand, as we strive, every check, every thwarting brings unpleasure, and final failure leaves us steeped in gloom.

The principle here enunciated is widely recognized as a general law of conation, even by some of those who accept also the hedonic principle. James Ward, for example, made subjective or hedonic selection fundamental in his psychology; he wrote: "The broad generalization already made may hold, viz. that receptive states lead through feeling to active states, and that those which are neither pleasant nor unpleasant, that neither please nor displease, meet with no responsive action[1]." Yet, he also wrote: "There is pleasure in proportion as a maximum of subjective activity or attention is effectively exercised, and pain in proportion as such effective attention is frustrated by restraints, distractions, shocks, or incomplete and faulty adaptations, or fails of exercise[2]." The former citation is a general statement of the hedonic, the latter of the hormic principle. Ward accepts both.

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[1] Dr E. Rignano is the only one who comes to my mind as explicitly maintaining a purely hormic view. Cf. his essay on "The Origin of the Affective Tendencies" in *Essays in Scientific Synthesis*. Perhaps I should place here the name of Dr C. G. Jung also.

[2] "A Great Advance of the Freudian Psychology," *Journ. of Abnormal and Social Psychology*, 1925, xx.

Prof. G. F. Stout approaches more nearly to a purely hormic psychology. He writes of "the intrinsic tendency of conative consciousness to find its own fulfilment." Further, "Feeling attitude in all its variations is most intimately connected with conation. We may distinguish three groups of cases. Under the first come all those phases of feeling—attitude which occur as episodes in the life-history of a pre-existing conation—all pains of disappointment or defeat and all pleasures of success and fruition....These feelings occur in connection with the various ways and degrees in which conative tendencies are being satisfied or dissatisfied, furthered or hindered.—In the second group of cases feeling-attitude and conation emerge coincidently, so that we cannot ascribe priority to either. In toothache the disagreeable consciousness and aversion coincide. Indeed, it seems super-subtle to make a distinction between them.—But, in strictness, the conative attitude of aversion is present from the outset, and the attempt to obtain relief is a development of it. In the third group of cases, feeling-attitude exists by itself without any appreciable intermixture of conative consciousness." He mentions instances of enjoyment that seem to be purely passive, and then adds: "But in such experiences, the conative attitude is always lurking, as it were, behind the scenes, ready to emerge at once, if the pleasure-giving conditions are in any way interrupted or discontinued before satiety is reached.. the reason why conation fails to appear while the pleasure-giving condition continues is that it is continuously merged in its own satisfaction. There is always a potential conation[3]."

In another place[4] Stout writes: "There are two ways of being interested; the affective and the conative. Affection and conation usually go together and are blended in intimate unity. None the less it is important to distinguish them, for affection covers the passive side of our nature as conscious subjects and conation the active side." Further:

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[1] *Psychological Principles*, p. 244.

[2] *Op. cit.* p. 262.

[3] *Groundwork of Psychology*, chapter III.

[4] *Manual of Psychology*, 3rd ed. p. 112.

"All pleasures and pains which are conditioned by success and failure belong to this type. They presuppose conation and cannot therefore be identical with it or even be regarded as generating it. Of course, there are also many instances in which pleasure or pain is primary, and not dependent on a prior want. When this is so, the conative attitude arises in dependence on the affective." Stout thus clearly formulates both the hormic and the hedonic principles and accepts both.

In this paper I am concerned to question the propriety of holding to both principles; and, since the hormic principle seems indisputable in many instances, to see whether we cannot reduce the apparent exceptions to conformity with it.

It is the instances of Stout's second group that are difficult to harmonize with a thoroughly hormic psychology and which give plausibility to the hedonic view; namely, the pleasures and pains of sense. The expression, pleasures and pains of sense, covers two broadly distinguishable classes of feelings, namely the aesthetic feelings proper and the sense-feelings; or more crudely, the pleasures and pains of perception and those of sensation. The difficulty I am concerned to consider does not arise in the former class; perceptions are conative activities, and all strictly aesthetic pleasures and pains, those of perception, may be readily brought under the hormic formula, as follows: We find pleasure in the object of perception in so far as its nature is such as to evoke a perceptive activity that is full, rich, and adequate to the object. The activity of perception brings unpleasure in so far as the object is too complex to be adequately apprehended through our perceptual organization (as in the case of rhythms whose complexity exceeds the degree that we have learnt to appreciate) or presents relations which hinder rather than facilitate the perceptive activity, such as sudden unexpected angles, discords, imperfect metre, all departures from the general form which our perceptive activity is pursuing.

It is then only the lower aesthetic feelings, or the feelings that accompany simple or crude sense-impressions, that present difficulty for the hormic theory and give plausibility to the hedonic theory. Upon simple inspection, what is called pain-sensation does *seem* to be intrinsically unpleasant; the same *seems* to be true of certain odours and tastes, while some tastes and odours, some contacts and tones, *seem* to be intrinsically pleasant.

But on reflection on some of these instances we find clear evidence that they conform to the hormic, rather than to the hedonic principle.

Consider first the case of sweet substances. It may be said that for the normal human being, and apparently for many animals, the taste of sugar is intrinsically pleasant. But the proposition is, I think, demonstrably false. I submit that the taste of sugar is pleasant only in so far as it promotes, renders more intense and effective, the conative impulse to consume food, and that the taste itself (the cognitive fact) does this, while the feeling depends upon and is consequential upon the conative process. For sugar is pleasant only when and in so far as the hunger-impulse is at work. As soon as satiety is reached the sweet taste of sugar becomes unpleasant; we cease to desire the sweet food, and, if it is forced upon us (as by the obligation to respond to an over-zealous hospitality), we find the food unpleasant, though still sweet. Here we have, as it seems to me, the clearest possible evidence of the dependence of feeling upon conation even in the case of very simple sense-perception. Sugar is, I submit, a specific excitant of the hunger or food-taking impulse, and is pleasant only in so far as it promotes the activity of digestion of food. It is a common experience with me that, if I am a little below my normal health, my appetite is apt to fail while I eat my morning roll or toast. The addition of a little marmalade then stimulates my impulse anew and I can take more food and find satisfaction in doing so.

It is commonly asserted that sweet quality of sensation is intrinsically pleasant, or that sweet sensation has the attribute of pleasure or has pleasant feeling-tone. To make such a statement is to commit the error of illegitimate abstraction which has pervaded and perverted so many discussions of sensory experience. To say that sweetness is a sensation, an element of consciousness, which evokes a pleasure-feeling, a second element of consciousness, is to commit the same error in a more flagrant manner. We must abandon such false abstraction, the root of so much error in psychology. We must recognize that all experience is activity; that in all sense-experience, even in what we crudely call 'having a simple sensation,' we are dealing with a perceptive activity of the organism. The concrete fact should be described by saying that when I eat sugar, I enjoy it, if I have appetite for it; and, if I do not have appetite, I do not enjoy it. The enjoyment, which in the language of false abstraction we describe by saying that an element of pleasure is added to the element of sweetness, is a qualification of the whole activity of eating the sugar. And the truth (obvious but commonly ignored) is that the enjoyment depends upon, is conditioned by, is a function of, not the quality of sweetness, but of the intensity of the conative process. The pleasantness of eating sugar is, then, but a special case of the general law that eating is pleasant in so far as it progressively satisfies the impulse to eat, an impulse which varies in intensity with the organic condition we call appetite or hunger and which in general is stimulated by sweet-tasting substances.

The same fact seems to be as clearly illustrated by stimulation of the specifically sexual areas of the skin. In so far as the sex impulse is aroused and operative, such stimulations are pleasant in a high degree; for they accentuate or intensify the impulse and promote the whole train of activity. But, if and when there is no sexual appetite and the sex impulse is latent, similar stimulations have no pleasurable effects.

Among sensory stimulations that seem to be intrinsically unpleasant, certain odours and all stimulations that produce 'physical pain' seem to offer difficulty to the hormic view. It must be admitted that in these cases feeling-attitude and conation seem, as Prof. Stout has said, "to emerge coincidently, so that we cannot ascribe priority to either. In toothache the disagreeable consciousness and aversion coincide." But Stout correctly adds that "the conative attitude of aversion is present from the outset"; and if we pay close attention to instances of the sudden onset of such stimulations and also to the obvious biological functions of our susceptibilities to them, we find evidence strongly in favour of the hormic view, the view that feeling is secondary to and dependent on conation.

The natural response to the stimulus that evokes 'pain sensation' is immediate withdrawal of the stimulated part. This seems to be the primary and essential function of this susceptibility; a part of the body deprived of this susceptibility soon becomes the seat of many injuries. I submit, then, that pain-sensation immediately evokes the impulse to withdraw, and that in so far as this impulse immediately attains its goal, the total process is not unpleasant. It is when the aversion, the impulse to withdraw, fails to terminate the sense-stimulation, that the process becomes unpleasant or painful in the true sense. We know how under continued evocation of the 'pain-sensation' we continue to make efforts to escape it, we wring the injured part, or rock to and fro, or march up and down in a painful, because fruitless, effort to get rid of the pain-exciting stimulus.

I think one may go further and say that the aversion evoked by 'pain-sensation' is the conative impulse of fear. My own introspection, many times repeated, has convinced me that any sudden 'pain-sensation,' excited either by external or internal stimulation of sensory organs or nerves, excites in me a momentary fear, so faint and fleeting as to be hardly recognizable. And we know how in animals and young children pain-exciting stimuli seem universally to excite fear.

'Pain-sensation' may then be brought under the hormic formula. Here again it is false abstraction that alone gives plausibility to the hedonic formulation. We falsely abstract the sensation and the feeling, and say that a 'pain-sensation' has the attribute of unpleasantness or evokes an element of painful feeling; whereas the true and concrete description of the fact runs as follows: the stimulation of certain sense-organs or nerves (more especially the so-called pain-spots or organs) excites an aversive impulse, an impulse to withdraw, to escape from the situation, and in so far as this impulse fails to attain its goal, the process, the activity, is unpleasant; and the more intense the impulse, the more unpleasant is the whole experience.

Here again, as in the case of sugar, there is no invariable correlation between the quality of the sensation and the hedonic tone. It is generally admitted that the quality of sensation called pain or *Stichempfindung* is not unpleasant in its fainter instances, but may enter into a pleasurable experience, as in a general tingling glow. And if we are for some reason interested in the sensory experience, if we wish to investigate it, if we regard the situation as beneficial, if we welcome and accept the stimulation (as, for example, the surgeon's needle) the unpleasantness fails to appear. That the algedonic quality of experiences in which 'pain-sensation' is prominent is dependent upon, is a function of, conation, is shown by fanatics who voluntarily gash and bruise themselves without sign of disagreeable feeling, by martyrs who, in the exaltation of religious aspiration, accept the physical injuries inflicted upon them as contributing to their supreme goal, the glory of God or their own salvation. And the same truth is illustrated by certain rare pathological cases, persons who under some morbidly intensified impulse will mutilate their bodies without sign of suffering. This occurs not only in extreme cases of mental disorder, but also in some cases of neurosis[1].

Pathology affords support for the same view in another class of cases, namely, the cases of neurotic pain localized in various bodily organs. Here we have indisputable instances in which pain localized in the body is due to no stimulation applied to sense organs or nerves, but rather to the conflict of strong conative tendencies, the pain of the conflict being projected upon some bodily organ, for reasons which in some cases remain obscure but which in others can be assigned.

Ache-sensation seems to me to be a specific sense-quality with specific function; namely its function is to excite an impulse to change our position and thus to prevent the ill consequences of too long continued maintenance of one posture with the serious trophic consequences which we see in the extreme form in the bed sores of persons of greatly reduced vitality, but which in less extreme cases take the form of local congestions. Normally, we change our posture as soon as ache-sensation attains sufficient intensity to excite this impulse. But if, for any reason, we have to inhibit the impulse, or if the change of posture fails to cut short the ache-sensation, or is impossible by reason of a splint and bandages, the experience becomes very disagreeable or painful in the proper sense.

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[1] Dr Macfie Campbell has recently informed me of one such case in which the patient, a girl who showed no grave mental disorder and who would be classed as merely somewhat hysterical, picked away pieces of her ears until little was left.

The case of unpleasant odours is closely parallel to that of 'pain-sensation.' The primary and universal function of such odours is to evoke aversion, to turn the creature aside from objects emitting such odours. And normally the impulse succeeds immediately in cutting short the impression, which then, in the limiting case, is hardly unpleasant. But if, for any reason, the sense-impression continues in spite of the aversive effort, the whole experience becomes highly unpleasant, and the aversive effort develops into the movements and attitudes of disgust; and in so far as the effort is still unsuccessful the experience is unpleasant, and unpleasant in proportion to the intensity of the aversive impulse. Here again, if we have some derived interest in the ill-smelling object, perhaps scientific curiosity, or regard it as a clue to some eagerly sought goal, say a buried treasure, the conation of this larger interest over-rides the instinctive impulse of aversion which is the primitive and simple response to the odour, and we do not find the total experience unpleasant, but rather pleasant, in so far as it leads on to our goal.

Besides the sense experiences indicated or implied in the foregoing discussion, there is one other case that remains as a difficulty for the hormic theory and a support for the hedonic theory, namely, certain very simple sense-impressions that seem to be intrinsically pleasurable, such as pure tones and pure colours. An object presenting a patch of pure colour may be found pleasant to contemplate; and the sound of a tuning fork is perhaps commonly found to be faintly pleasurable. But even here we have to do with perceptive activities, rather than purely passive reception of impressions. The psychologists of the *Gestalt* School have taught us that even a simple patch of colour or a simple tone cannot properly be treated as an isolated sensation, that rather, the colour or the tone always appears to us as standing out from a background, that is, as a feature of a larger whole; or in other words, that here also we have to do, not with an isolated sensation, but rather with a perceptive activity. And in such cases the pleasantness of the clearly bounded patch of colour must, I submit, be regarded as due to the fact that the colour facilitates the perceptive activity, enables us to distinguish readily the object from its ground. The truth of this view becomes apparent if we complicate the coloured area, making of it a repeating pattern; then, in so far as the pattern does not become too complex for assimilation by our perceptual organization, each increase of complexity adds to the pleasantness of the perceptive activity, because rendering it fuller and richer.

Similar considerations hold good for the pure tone. The clear-cut quality is faintly pleasurable because it promotes ready discrimination of tone from background; and if we add other tones, the total activity of perception becomes more pleasant in so far as the additional tones are in relations that are readily apprehended, relations of quality and rhythm such as lend themselves to complete perception, such as facilitate the perception of the whole complex or series as a distinctive object, a clang or a melody.

On the other hand a surface covered with ill-defined patches of colour, having no definite or significant relations, qualitative or spatial, is found disagreeable, because our perceptive organization cannot cope with it; and a complex of inharmonious tones or a series of tones in no orderly relations of quality and rhythm is disagreeable because we have no perceptive organization that can cope with, can assimilate, the complex. This last was vividly illustrated for me by the experience of hearing the beating of rhythms on gongs by Malay musicians. These people, having only very restricted means of producing tonal combinations, have relied upon rhythm more largely than European musicians do, and seem in consequence to have developed to a high pitch their perceptive organization for appreciation of rhythm. Hence they find pleasure in rhythms which are so complex as to be for the European hearer distinctly unpleasant, because they exceed his power of coping with them and appear to him as merely confused noises.

I have now dealt with the types of experience that seem to present most difficulty for the hormic theory and lend plausibility to the hedonic theory. If I have succeeded in showing that these can be brought under the hormic formula, I do not think that any other types need be regarded as resistant to similar investigation.

But, it may be said, is it not plain that common sense and scientific opinion agree in accepting certain sensations as pleasant and others as unpleasant? Why exert a perverted ingenuity in an attempt to upset this view, one of the few fixed and generally accepted formulations that psychology can boast of? My answer is, first, that the validity of the hormic formulation seems clear and indisputable in regard to our more developed activities, and it seems highly improbable that nature should have worked upon two distinct and opposite principles, one of them valid throughout the lower range of experience, the other throughout the more developed ranges. It is in respect of the latter that we can best teach a clear well-founded formulation; study of them points unmistakeably to the hormic principle. The fact justifies and demands that we review the lower ranges of experience in the light of this guidance, and, as I have tried to show, such review leads to the conclusion that the hormic principle is valid throughout, and that hedonism is a product of false or vicious abstraction exerted upon those facts of experience, the so-called sensations, which present a misleading appearance of simplicity, but which in reality are the most obscure and baffling to all psychological enquiry.

I would add that the prevalence of the hedonist view is largely due to the fact that feeling is always insistent in consciousness, is not easily overlooked, whereas conative experience is very little differentiated and always obscure to introspection, so obscure that the majority of psychologists overlook it completely, if they do not positively deny it. And this is especially true of the lower and simpler forms of conation, those involved in the simpler acts of sense-perception. But modern psychopathology is revealing more and more clearly the importance and the multiplicity of these obscure conations; it is showing how such obscure conations underlie our moods and take part in subconscious conflicts which contribute to consciousness only their affective tone; it is showing, in short, that the algedonic tone of consciousness at any moment is commonly the resultant of many obscure conations only partially integrated within the total system of activity.

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THE DEVELOPMENT OF SENTENCE STRUCTURE IN CHILDHOOD

By WILLIAM BOYD.

I. *Introduction* (pp. 181–183).
II. *Sentence forms* (p. 183).
III. *Clause construction* (pp. 183–185).
IV. *Elements in the vocabulary* (pp. 186, 187).
V. *The length of sentences* (p. 187).
VI. *The special idioms of the child* (pp. 187–191).

I. *Introduction.*

THE best method of getting material for the study of a young child's speech is to note down sequences of ordinary unselected utterances on a variety of occasions. The child uses different words and frames different kinds of sentences with every change of environment. He talks in one way when busy about his own concerns, and in another when conversing with other people. The sentences that issue from his lips when he is re-telling a familiar tale have a quality that is lacking when he is speaking of happenings for which he has to find new words. In the course of the day's routine there are many variations in emotional intensity which reflect themselves in changes of language. What is needed is a sample of the child's speech sufficiently varied to include all the more typical forms of expression. Such a sample can best be obtained by a verbatim record of series of remarks made when the child is at play, at meals, on a railway journey, out walking, on visits to friends, at the story hour, engaged in soliloquy, or, indeed, at any period except during prolonged spells of emotional disturbance.

It was on these lines that the material for the present study of my little daughter's sentences was obtained. Each year a collection of 1250 unselected sentences was compiled round about her birthday, usually during the fortnight before and the fortnight after. The first collection, which was exceptional in that it included sentences gathered over four months, was completed by the end of the second year. The seventh and last was made just before and after the ninth birthday.

In the absence of any later records of this particular child's speech, a hundred conversational sentences have been taken at random, for purposes of comparison, from each of ten male novelists—Trollope, Scott, Hardy, Jack London, Stevenson, Barrie, Bennett, Kipling, Wells, Gibbs—and from each of eight female novelists—Jane Austen, Susan Ferrier, Gene Stratton Porter, George Eliot, Mrs Ewing, 'Elizabeth,' Mrs Humphry Ward, Kathleen Norris[1]. Words and sentences occurring in the conversational passages of novels possibly differ considerably from those that occur in normal adult conversation, but lacking any records of normal adult conversation at once typical and adequate and with no possibility of obtaining them one may take these 1800 conversational sentences as an approximation to real conversation and use them to bring into relief some of the distinctive features of childish speech.

Before proceeding to a detailed analysis of the child's sentences, the general character of the development which took place during the period under survey may be illustrated by typical conversations at two, four, six and eight years of age, all but the first taken down in the course of a railway journey.

AT TWO YEARS: *A Dinner Conversation.* Isa wanting potatoes in it. Wanting down. Daddy hold that. That (*is*) potatoes. That (*is the*) potato spoon.' Plates not there. Wanting more potatoes. Isa eat it. Help a wee girl big chair. Nelly help a wee girl. Put some potatoes out (*to the*) wee birdies. There's Bobby. Put some out (*to the*) pigeons. Put some out (*to the*) nice pigeons. Isa putting (*it*) out (*at the*) backside door. Isa wanting in there. Isa wanting in bed. Want see Aggie's. Wait (*till*) Isa gets down....

AT FOUR YEARS: *In the Train.* Daddy, where's the little bag? Is that the golf course? I can remember the station. Daddy, let me up on your knee. Tell me if the flag was not there. I was hiding not to get the nasty smoke from the tunnel. It is only the little baby sheep that are frightened. I've hardly time to see any thing. It's going so fast, the train. Look at the lovely flowers. Daddy, is that nobody's whin? I could sit on that bank and pull such a lot of whin. I would like to have gloves. It must be called Thornliebank, because there is a bank there. Do you know what my baby calls Thornliebank? She calls it bank near the station. Which Cart? Whose is that garden? What is that little burn for?...

AT SIX YEARS: *In the Train.* Where does this train go to, mother? Look how the blood's come through. What's L.S.W.R. for? It is on a waggon. I think I might have off my bonnet. My head's so hot. Do we pass Montrose again? Mother, we are going backwards. Where's the bag? I am going to this side because he pulled up the window. It is a higher side and a better side. Daddy just came across to that side, and I went across to that side. Oh, see the wee pet, and a father horse and a mother horse. There's woods. May I put on my gloves now? Put on my fur now. Oh, that hill is lying white with snow. Another bonny wood. They are only that size. This is the side they take the tickets....

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[1] The two groups of writers are here set down in accordance with the average number of words in the sentence beginning with the writer whose sentences are longest and in that and other related respects most different from the child.

AT EIGHT YEARS: *In the Train.* I wish the train would hurry up. Come, because it will maybe swish you away. I hate the train when it comes roaring in. Please put that up on the rack. I gave Cordelia and my dolls a kiss. Cordelia just sits all the time. There's where Lily and I sometimes go for a short cut. I remember once a whole lot of our girls were planning to go that way. Mr Clark never said anything, but they just turned and fled. If only you had taken a little more time and not rushed so much. Mostly all ladies—don't care for smoking. Daddy, how did they manage to make up such a good story as *The Days of Chivalry*? It is very difficult. Do you know what I am doing? Lily and I play at making stories about witches and magic catkins. I just make it up as I go along. It is not very easy. I never think the main thing. I just think it out as I go along. I find it much easier. I enjoy a barrow ride better than a train ride. How is that?...

II. *Sentence forms.*

There are four kinds of sentences: statements, questions, commands and exclamations. The percentage of these, (*a*) in the speech of the child in successive years, and (*b*) in the conversational sentences chosen from the novelists, was as follows:

TABLE I.

	Statements	Questions	Commands	Exclamations
II.	79·9	2·0	17·1	1·0
III.	62·4	28·8	8·6	0·2
IV.	63·2	26·8	9·0	1·0
V.	65·8	24·6	8·9	0·6
VI.	67·7	21·4	10·2	0·7
VII.	69·7	23·8	5·7	0·8
VIII.	69·4	24·2	5·3	1·1
Women	71·2	18·9	7·9	2·0
Men	76·4	15·5	6·6	1·5

It will be seen that after the exceptional second year, the percentages of categorical statements in the child's speech increases steadily towards the adult proportion; that on the average, though not in every individual case, questions are more frequent in childish speech than in adult (the mean deviation for the men is 6·1, and for the women 2·9); and that imperatives begin by being more frequent in the child than in most of the adults and by reason of a sharp drop in the last two years become less frequent by seven and eight years of age.

III. *Clause construction.*

The development of sentence structure as the child advances in years assumes two main forms. On the one hand, there is a process of aggregation, a linking up of sentence with sentence by means of 'and,' 'but' and similar connective words, which results in an increase of compound sentences. On the other hand, there is a process of complication which manifests itself in an increasing use of subordinate clauses (noun, relative and adverbial) and of sub-subordinate clauses relating to other subordinate clauses. Reducing details to a minimum we may distinguish (and symbolize) the main types of sentence construction thus:

PxPy	Two or more principal clauses, each with a subordinate clause, or one with a subordinate clause to which is attached a sub-subordinate clause.
PPx	Two or more principal clauses, one of which has a subordinate clause connected with it.
PP	Two or more principal clauses without subordinate clauses.
Pxy	A principal clause with two subordinates, or with a subordinate and sub-subordinate.
Px	A principal clause with a subordinate clause connected.
P	A principal clause and nothing more.

The percentages of these types (*a*) in the child's speech in successive years and (*b*) in the novelists' conversations, was as follows:

TABLE II.

	PxPy	*PPx*	*PP*	*Pxy*	*Px*	*P*
III.	—	1·0	5·6	0·5	11·5	81·4
IV.	—	0·9	4·8	0·8	14·8	78·6
V.	0·5	1·8	7·2	1·6	17·8	71·0
VI.	0·6	1·8	6·1	1·9	15·8	73·7
VII.	0·6	2·7	7·2	2·8	17·3	69·4
VIII.	0·4	2·7	6·8	2·4	17·7	70·0
Women	2·6	6·0	10·9	6·6	18·5	55·4
Men	1·1	4·4	8·7	6·8	20·6	58·5

This is a very interesting table. It seems to show that after the age of four the progress made towards adult performance in sentence construction takes place by complication rather than by aggregation. The proportion of sentences of the *PP*, *Px* and *P* types remains fairly steady for the last four years under consideration. Advance is registered in the more elaborate *PxPy*, *PPx*, and *Pxy* types. The difference between child and adult is well marked both in respect of aggregation and complication.

The development of clause construction may be considered from another angle. Complication depends on the use made of subordinate clauses. It is interesting to see what this implies in regard to the relative frequency of the different forms of subordination. The simplest comparable expression of this is the average number of noun, adjectival and adverbial clauses per hundred principal clauses:

TABLE III.

	Noun clauses	Adjectival clauses	Adverbial clauses
III.	4·8	2·8	6·6
IV.	7·5	4·2	9·4
V.	9·8	4·5	11·5
VI.	10·2	4·1	10·5
VII.	12·7	5·0	13·1
VIII.	12·5	3·8	13·7
Women	15·5	4·8	17·2
Men	15·5	6·0	15·5

Considering only the proportionate occurrence of the three forms of subordination, we find the relative percentage of noun, adjectival and adverbial clauses to be as follows:

TABLE IV.

	Noun clauses	Adjectival clauses	Adverbial clauses
III.	34	20	47
IV.	37	19	44
V.	38	18	44
VI.	41	17	43
VII.	41	16	43
VIII.	42	13	46
Women	41	13	46
Men	42	16	42

The increasing importance of the noun clause as the child's speech changes with the years comes rather as a surprise. That relative clauses should fall in comparative importance might perhaps have been anticipated: at any rate it does not appear strange. But on an *a priori* judgment one might have expected the adverbial clauses, with its implication of many-sided inter-relation of ideas, to have enjoyed a growing prominence in this phase of language development. The adverbial clause is important, of course, but in a stationary manner. It is the noun clause, not the adverbial, which develops greater importance in speech. Perhaps the explanation is to be found in the fact that it is generally words like 'say,' 'think,' 'ask' and others of kindred meaning that lead on to the 'that' or the 'what' of the noun clause. Is the increasing consequence of the noun type of subordination not just an index of the gradual enrichment of the social and personal experience, as the child begins to pass beyond the life of the home into the bigger and deeper life of later childhood[1]?

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[1] It may be added here that an analysis of the compositions of 83 elementary pupils (boys and girls) of average age 12 gave the relative percentage of noun, adjectival and adverbial clauses as 21, 22, and 57. The adverbial clauses were nearly all clauses of time.

IV. *Elements in the vocabulary.*

Coming now to the words which are the components of clauses and sentences, we may consider in the first place the relative percentages of the different parts of speech in the vocabulary of the collected sentences of the child[1] (counting each word only once):

TABLE V.

	Nouns	Pronouns	Adjectives	Prepositions	Verbs	Adverbs	Conjunctions	Interjections
II.	49·2	2·7	13·6	2·4	22·3	7·0	1·1	1·5
III.	43·5	3·1	16·5	2·8	22·2	9·1	2·2	0·4
IV.	42·2	3·6	16·8	3·0	22·2	8·8	2·7	0·7
V.	44·8	3·3	16·8	2·6	20·0	9·1	2·6	0·9
VI.	43·8	3·2	17·0	2·7	21·7	8·2	2·6	0·8
VII.	42·3	3·0	18·7	2·5	22·0	8·6	2·6	0·4
VIII.	45·6	2·8	17·1	2·4	20·9	8·1	2·2	0·9

The most significant fact here is the general steadiness of the proportions of the different elements of the vocabulary from the age of three onwards.

Consider next the relative percentages of the different parts of speech in the total words of the collected sentences of the child and of the novelists' conversations (counting each word every time it occurs):

TABLE VI.

	Nouns	Pronouns	Adjectives	Prepositions	Verbs	Adverbs	Conjunctions	Interjections
II.	36·8	6·2	13·7	1·4	28·2	13·2	0·2	0·3
III.	16·4	15·8	17·4	7·9	29·6	9·4	3·4	0·2
IV.	14·8	18·1	14·6	7·6	30·0	11·6	2·9	0·3
V.	14·7	18·7	15·5	6·9	29·6	10·1	4·0	0·4
VI.	15·3	17·6	15·8	7·7	28·6	10·1	4·4	0·4
VII.	14·3	18·5	15·4	7·3	28·2	10·8	5·0	0·5
VIII.	14·6	18·4	15·2	7·7	28·3	10·2	5·2	0·3
Women	15·1	17·9	16·1	9·0	24·5	9·6	6·8	1·1
Men	16·3	17·2	16·7	9·2	24·2	9·2	6·6	0·6

Once again the steadiness of the proportions of the different parts of the child's speech in successive years deserves notice. From four onwards they are remarkably stable. But the point of special interest is in the comparison with the two sets of figures for the novelists' conversations. The percentage of the relational words (conjunctions and prepositions) in the adult sentences is quite definitely higher, as one might expect. The percentage of the verbs and of the adverbs associated with them, on the other hand, is considerably lower. In actual fact, in no single novelist does the percentage of verbs come near the lowest relative figure on the child's list. Nouns and pronouns taken together are of nearly equal frequency in child and adult, and at all ages of the child after two, but the adult tends to use more nouns and fewer pronouns.

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[1] No attempt was made to get the corresponding figures for the conversational sentences of the novelists. It would have been interesting to have had these figures, but the labour involved seemed out of proportion to the value of the results.

V. *The length of sentences.*

One of the clearest signs of linguistic progress in childhood, as Mrs Nice has shown[1], is increasing length of sentences. My figures confirm this view. The average number of words per sentence for the seven years under review was 3·3, 6·4, 6·9, 7·5, 7·7, 8·2, 8·5. This falls considerably below the average (and mean deviation) for the novelists' conversations, which was 11·1 ± 1·7 for the female novelists and 10·0 ± 1·5 for the male novelists, but probably not greatly below the average for ordinary conversation.

At this point may be noted the fact that up to the age of five there is a gradual increase in the number of times the individual words in the child's vocabulary (represented in the collected sentences) are used, and then comes a gradual decrease. The average number of times the words in the vocabulary are used in the seven successive years is: 6·3, 8·1, 8·4, 8·8, 8·2, 8·0, 7·7. (There are no corresponding figures for the novelists' conversations.)

VI. *The special idioms of the child.*

At two practically every sentence the child utters is distinctly childish. A year later only some 90 sentences out of 1250 differ markedly enough from the sentences of a grown-up person to draw attention to their infantile origin; and by the age of eight the number of unusual sentences is down to 45 in 1250 or thereabouts. To see the significance of the child's departures from grown-up usage, as far as that is possible with the scanty material available, we will take as starting-point in our study of the idioms of childhood the outstanding features of the two-year-old speech and try to follow their development in the later years.

The first point that calls for note is *the dearth of prepositions, conjunctions, articles and auxiliary verbs* at two. These important elements of speech only constitute 3·4 per cent. of the total words used at two as compared with 25 per cent. a year later, and at the age of eight. The lack of prepositions and conjunctions is of special consequence for sentence structure. With only a few prepositions in use the formation of qualifying phrases which gives its characteristic elasticity to the English language is much restricted; and the almost entire absence of conjunctions of any kind makes the expression of co-ordinate or subordinate relationships practically impossible. In actual fact the child mind triumphs over the limitations of vocabulary, and succeeds in hinting at the prepositional phrase without using prepositions in 13 cases—*e.g.* 'Get more milk [*from the*] farm'—and at subordinate relations without using conjunctions in 55 cases—*e.g.* 'Wait [*till the*] eggs [*are*] in.'

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[1] "Length of Sentences as a Criterion of a Child's Progress in Speech," *Journ. Educat. Psychol.* September, 1925.

By the time the child reaches the age of three the difficulty has almost completely passed. Articles and auxiliaries are no longer omitted, and with a few exceptions all the prepositions and conjunctions are at command. Progress does not consist in the addition of new words so much as in the freer use of those already acquired, as indicated by the increasing number of subordinate clauses relatively to the principal clauses (*see* Table III), and in the rising percentage of conjunctions in the total words used (*see* Table VI).

There is next to be noted *the frequency of elliptical constructions* at two, quite apart from those consequent on the paucity of prepositions and conjunctions. Only 30·6 per cent. of the child's sentences at this age are fully expressed predications with explicit subject and predicate. Even when we add the 14·3 per cent. of 'imperative' remarks, which would be normally elliptical in adult speech, there still remain 55·1 per cent. of the child's sentences with subject or predicate omitted or curtailed. In more than half of these, the subject, or subject and copula, is lacking, *e.g.* '[*Let Isa*] put on mother's shoes,' '[*It is*] hot.' The next largest group has no copula, *e.g.* 'That [*is the*] train,' 'That wee boy [*is*] sleeping.' A few have abbreviated predicates, usually with an adverb functioning as a verb, *e.g.* '[*The*] book [*has fallen*] down.'

The practice of elliptical speech lingers on into the third year in one or two special forms. 2 per cent. of the sentences at three are elliptical. The most common type—not really very common, since it only occurs 15 times in all—is a sentence with the first personal pronoun omitted: *e.g.* '[*I*] think maybe her mother might comb it,' '[*I have*] got a wee knock.' This practically disappears after three, though curiously enough there are two examples of it so late as seven. A second type which occurs 6 times at three, and 11 times at four, and appears once or twice at six takes the form, 'Tell me if—,' 'What if—' so-and-so took place: *e.g.* 'Tell me if I don't find any,' meaning 'Tell me what will happen if I do not find any.' A new kind of ellipsis which makes its first appearance at three and goes on increasing in frequency till it ceases after six is the broken sentence,—*e.g.* '—Sewing, and then they got out to play'—uttered in response to someone's remark and borrowing its beginning from it.

A third interesting feature of the two-year-old speech is *the amplification of a remark made.* In 20 sentences the predicate is used without the subject, and immediately afterwards the sentence is repeated with the addition of the subject: *e.g.* 'Going up drawing-room. Isa going up drawing-room.' Sometimes it is the predicate which receives extension, *e.g.* 'See cows. See cows running [*at*] that farm.' And sometimes an article or a preposition is added: *e.g.* 'Lizzie get milk-cans girl. Get milk-cans from girl.' Amplification of one kind and another occurs about 70 times at two.

A similar idiom occurs in the speech of all the succeeding years: a statement being made and then expanded or corrected as an after-thought. There are 14 examples of this at three, 13 at four, 14 at five, 22 at six, 21 at seven, and 17 at eight. Here are examples at three, five and seven: 'The big girl gave it, that big girl with the black dog like Koko, very like Koko' (3); 'I want my book, the one with the wee elves all round it' (5); 'That's a biplane—with two wings' (7). A related idiom, which is specially noticeable in the sentences of the five-year-old is the postponement of the adverb to the end of the sentence, as if the qualification made had only suggested itself when the statement was on the way to completion without it: *e.g.* 'This would fit it maybe.' 'I am going to get a better one even.'

A fourth feature of the speech of two deserving of note is *an inversion of the usual order of speech* so as to put the fact that is first in mind in the forefront of the sentence. The most common and most striking form of this is the mention of the object word prior to predication: *e.g.* 'Pillar, put it (the pillar) down there.' 'Chair. Isa wants chair.' This occurs 40 times at this stage. In the second form of inversion, the subject brings up the rear, as sometimes happens in the process of amplification: *e.g.* 'Get fish, Isa.' 'Cannot come off, wee baby boot.' This particular form occurs 30 times.

The strange thing is that there are only one or two examples of inversion in the collected sentences at three and four years of age, and then at five come more than a score of them: 'Roses, I have got no roses,' 'Any tree that's got branches, he climbs it,' 'Whisky, did it make him sick?' At the same age, there is a kindred form of expression in which the so-called extension of the predicate is put before the definitive verb: *e.g.* 'It was in the grass I fell.' Then after five there is only an occasional inversion. It may be added, however, that at all ages from three to eight, there is an interesting tendency to introduce sentences by an adverbial word or phrase (rhetorically effective) which seems to have its origin in the same impulse to put the fact of major interest at the beginning of the sentence. Here are some examples from the speech of four: 'After the dishes, will you play?' 'Often I have seen little girls do that,' 'Last time when I went with my mother, was I not afraid?'

The last point to be noted about the child's speech at two is the very frequent *omission of grammatical inflections*. It is true that all the common inflections are represented in the two-year-old collection of sentences, but, with the exception of the present participle which is used with certainty throughout, they are very often omitted. There are some plural and possessive endings, but for the most part the uninflected singular form serves to denote both plural and possessive. Similarly the uninflected form of the verb which appears in the infinitive is in common use for the present indicative, both singular and plural, as well as for past and future and for some expressions of mood. The comparatives of adjectives occur a few times, but in a way that leaves one doubtful whether there is any real comprehension of what comparison implies.

A year later, all the ordinary grammatical forms are employed with as complete assurance as by any adult, and grammatical mistakes are rare. Now and then the child gets into difficulties with the construction of a sentence, as when she wants to know 'Is that the way how to make a triangle?' and sometimes unrelated statements are brought into verbal juxtaposition by means of an 'and,' as often happens with loose-thinking adults. But these are the unimportant exceptions. The rule is accurate expression in a grammatical sense, and so it continues through the years under review with one curious lapse at the age of six when several plural nominatives are yoked with singular verbs and the nominative and objective forms of pronouns once or twice get confused.

It is worth pointing out in conclusion how near the speech of the eight-year-old comes to the adult form. It has relatively a greater number of principal clauses, and (perhaps in consequence) a larger proportion of verbs, but the idioms with a few unimportant exceptions are the idioms of the adult. There is an occasional inversion. One or two longish sentences have got into a bit of a tangle. Some adverbs are in the wrong place in the sentence, either because the child has not yet acquired sufficient control of word order, or because the interest of the moment has created an unorthodox order of its own. There is quite a considerable number of sentences in which the initial statement has been extended and amplified as by a second thought. Even when all these are taken into account the fact remains that over 1200 out of 1250 sentences are perfectly ordinary sentences which any one might use. And though a few of the exceptions might be designated 'childish,' the majority may be regarded as simply the clumsy utterances of extempore speech, such as may be heard on the lips of most people now and then. It is clear that even with wide provinces of culture still to be mastered the child of eight has an almost adult command of the instrument of language.

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AN EXPERIMENTAL STUDY OF A REPETITIVE PROCESS

By S. WYATT.

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I. *Nature of the investigation* (pp. 192, 193).
II. *Results obtained* (pp. 193–205).
  (*a*) *Intelligence and working efficiency* (pp. 193–195).
  (*b*) *Motor ability and working efficiency* (pp. 195–197).
  (*c*) *Intelligence, working efficiency, and boredom* (pp. 197–202).
  (*d*) *Adaptation to monotonous conditions of work* (pp. 202–205).
III. *Discussion of results* (pp. 205–208).
IV. *Summary of conclusions* (pp. 208, 209).

I. *Nature of the investigation.*

In connection with an investigation on repetition work carried out in a soap factory, an attempt was made to determine the degree of dependence between working efficiency and the amount of intelligence possessed by the operatives. It has occasionally been asserted that repetitive processes are performed most satisfactorily by individuals endowed with a relatively low order of intelligence[1], because such individuals are not only capable of attaining a high rate of working, but are better able to endure the uniformity of the frequently recurring cycles of activity. In order to obtain some evidence in this connection, intelligence tests[2] were applied individually to 30 girls engaged in the process of soap wrapping and packing. Each cycle of movements consisted essentially of wrapping and packing three tablets of soap in a cardboard box, and occupied approximately 40 seconds. The process was accordingly repeated about 720 times in a day of eight hours, and from the standpoint of uniformity in both activity and posture constituted a typical repetitive process.

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[1] See, for instance, Report No. 30 of the Industrial Fatigue Research Board, pp. 8 and 9.

[2] The tests applied were Series No. 33 of the National Institute of Industrial Psychology. They consisted of (1) Opposites, (2) Analogies, (3) Mixed sentences, (4) Completing sentences, and (5) Reasoning. Each of the first three tests occupied three minutes and the last two ten minutes each.

In addition to the intelligence tests, an attempt was also made to determine the nature of the particular capacities involved in soap wrapping. An analysis of the industrial activities suggested that the most important of these were connected with manipulative speed and dexterity, and seemed to involve (*a*) finger dexterity, (*b*) accuracy of movement, (*c*) rapidity of movement and (*d*) sensitivity to touch. The following tests were accordingly devised and applied.

(*a*) *Knotting test.* This consisted of ten pieces of string each six inches in length, and the subject was requested to tie five ordinary knots on each piece as quickly as possible. The time taken to complete the ten pieces was taken as the test score. Speed and dexterity of finger movements appeared to be the chief abilities required for the successful performance of this test.

(*b*) *Match-stick insertion test.* This was similar to the one previously used by Muscio[1] and others, and consisted of five rows of 14 holes in a flat piece of wood in which the subjects inserted matches one at a time. The time taken to complete the 70 holes represented the test score. Success in this test seemed to be largely dependent upon accuracy and rapidity of movement.

(*c*) *Tapping test.* In this test the subject was required to tap for 30 sec. as quickly as possible with the first finger of the right hand. The number of taps so performed was taken as a measure of ability in this test. Rapidity of movement was the chief ability involved.

(*d*) *Touch discrimination.* In this test five steel balls were used, having diameters of 20, 21, 22, 23 and 24 sixty-fourths of an inch. The ball of medium size was first presented as standard and the subject picked it up between the first two fingers and thumb of the right hand. (Eyes closed or directed away from the balls.) The ball was then dropped into a bowl and another ball examined in a similar manner. The subject was required to say whether the second ball of the pair was larger, smaller, or of the same size as the first. In this manner each of the five balls was presented five times (in a definite but irregular order) immediately after the standard or middle ball, and the replies noted. One mark was deducted for each error of the first order, and two marks when the error was of the second place[2].

The tests were applied in a quiet room adjoining the department in which the girls worked, and occupied about an hour for each girl. The purpose of the tests was first explained to each subject, and the tests were then given in the order described above.

II. *Results obtained.*

(*a*) *Intelligence and working efficiency.* The sum of the scores obtained by each worker in the five intelligence tests has been expressed as a percentage of the average score for all the workers and the results are given in Table I. The efficiency figures, based upon the observed output over a period of eight weeks, have been treated in a similar manner and are also included in the Table.

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[1] Report No. 16 of the Industrial Fatigue Research Board, p. 10.

[2] Although the balls differed in weight as well as in volume and surface area, discrimination was performed almost entirely by means of touch.

TABLE I. *Scores representative of each worker in Intelligence* (I) *and Efficiency* (E).

Worker	I	E	Worker	I	E	Worker	I	E
1	205·9	117·2	11	109·9	97·5	21	76·8	112·0
2	192·0	117·0	12	104·7	98·2	22	76·8	98·4
3	157·1	86·0	13	104·7	96·3	23	68·1	113·4
4	151·8	96·1	14	94·2	104·0	24	59·3	88·3
5	143·6	101·1	15	92·5	95·2	25	57·6	112·4
6	143·1	91·9	16	90·8	94·8	26	57·6	91·3
7	122·2	87·5	17	90·8	93·8	27	57·6	99·6
8	118·7	99·0	18	89·0	108·6	28	52·4	92·1
9	116·9	93·8	19	89·0	109·0	29	52·4	99·2
10	115·2	109·7	20	82·0	110·9	30	29·7	86·2

The rank of each individual in the two series under comparison is also shown in the following curves.

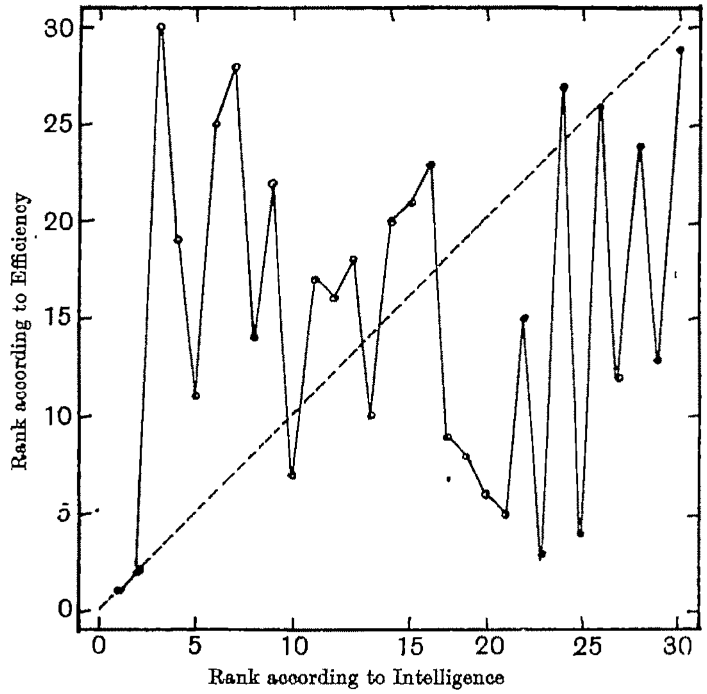

Fig. 1. Relation between Intelligence and Efficiency.

If a perfect relationship existed between intelligence and working efficiency, the points of the curve would all lie along the broken line. The scattered nature of the points shows at once that the degree of similarity between the two abilities under consideration is very slight. (The coefficient of correlation 'r' is + 0·25 ± ·114.) A consideration of the results shows that although highly intelligent operatives (*e.g.* nos. 1 and 2) may be very efficient workers, a high output may also be obtained from workers with a low order of intelligence (*e.g.* nos. 21, 23, and 25). Similarly, workers who are much above the average in intelligence (*e.g.* nos. 3 and 4), may be below the average in working efficiency.

If the workers are divided into three equal groups according to their order of intelligence, and the average intelligence and efficiency scores determined for each group, we obtain the following results:

	Workers 1 to 10	Workers 11 to 20	Workers 21 to 30
Average intelligence	146·7	94·8	58·8
Average efficiency	99·9	100·8	99·3

Thus the workers of inferior intelligence (nos. 21–30) are just as efficient in the industrial task as those of average (nos. 11–20) or superior intelligence (nos. 1–10). In general, therefore, it appears that the amount of intelligence (as measured by the tests used) possessed by an individual is practically no criterion of her efficiency as a worker in a simple repetitive process of the type under consideration.

(*b*) *Motor ability and working efficiency.* Since efficiency in the process in question does not appear to be dependent upon general intelligence, an attempt was made to determine the extent to which efficiency was correlated with dexterity and rapidity of movement. For this purpose tests (*a*), (*b*), and (*c*) were applied[1], and were repeated after an interval of three months. Only 27 of the original operatives were available for the second application of the tests, three of the most intelligent workers (nos. 1, 2, and 11) having left to be married. The results obtained are given in the following table.

TABLE II. *Coefficients of correlation between manual tests and working efficiency.*

	1st test	2nd test	Both combined
Knotting	+0·03 ± 0·13	+0·20 ± 0·12	+0·12 ± 0·13
Match insertion	+0·03 ± 0·13	+0·05 ± 0·13	+0·06 ± 0·13
Tapping	−0·18 ± 0·13	+0·14 ± 0·13	0·00 ± 0·13
Touch	+0·21 ± 0·12		

The reliability coefficients between the results of the first and second applications of the same test are:

Knotting	+ 0·57 ± 0·09
Match insertion	+ 0·35 ± 0·11
Tapping	+ 0·68 ± 0·07

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[1] Test (*d*) was only applied once.

Thus the correlations between each of the manual tests and efficiency is quite insignificant. The reliability coefficients are also very small, and indicate considerable changes in the relative achievements of the same workers in each pair of similar tests.

It is interesting to note that a slightly higher correlation with efficiency was obtained in the second application of the motor tests, but the increase is not appreciable. The slight improvement in question is probably due to a decrease in nervousness when the tests were repeated, and further applications of the same tests might conceivably give higher correlations with efficiency.

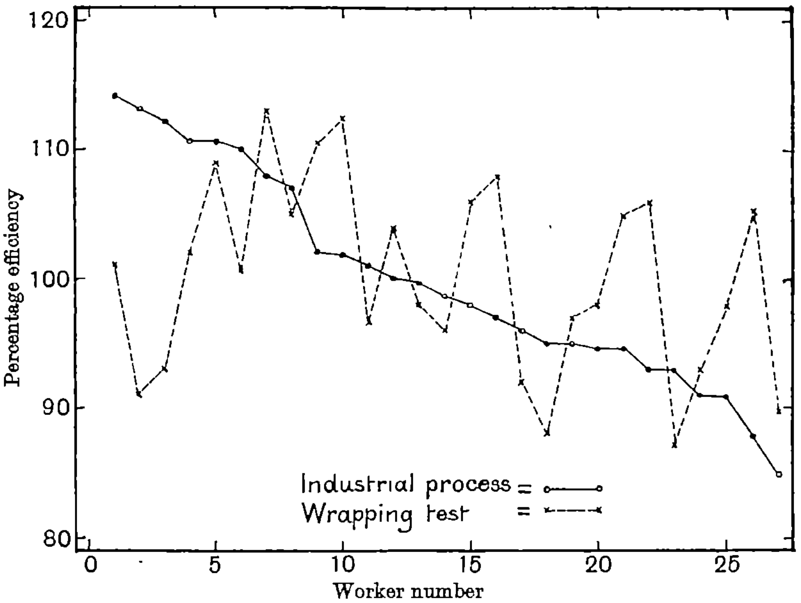


Fig. 2. Comparative efficiency of each worker in (*a*) soap-wrapping test, (*b*) industrial operation.

Since the above tests were found to be useless as a means of disclosing abilities involved in industrial efficiency, a modified form of the actual industrial process was given as a test. In this procedure the worker was requested to wrap 50 tablets of soap as quickly as possible. Both soap and wrappers were taken from the stock in general use, and were arranged on the table to resemble as far as possible actual working conditions. The coefficient of correlation (r) between the results of this test and working efficiency was $+ 0{\cdot}22 \pm 0{\cdot}13$. Thus a standardized form of the actual industrial process, when presented under test conditions, fails to give any appreciable correlation with the same operation when performed under ordinary working conditions.

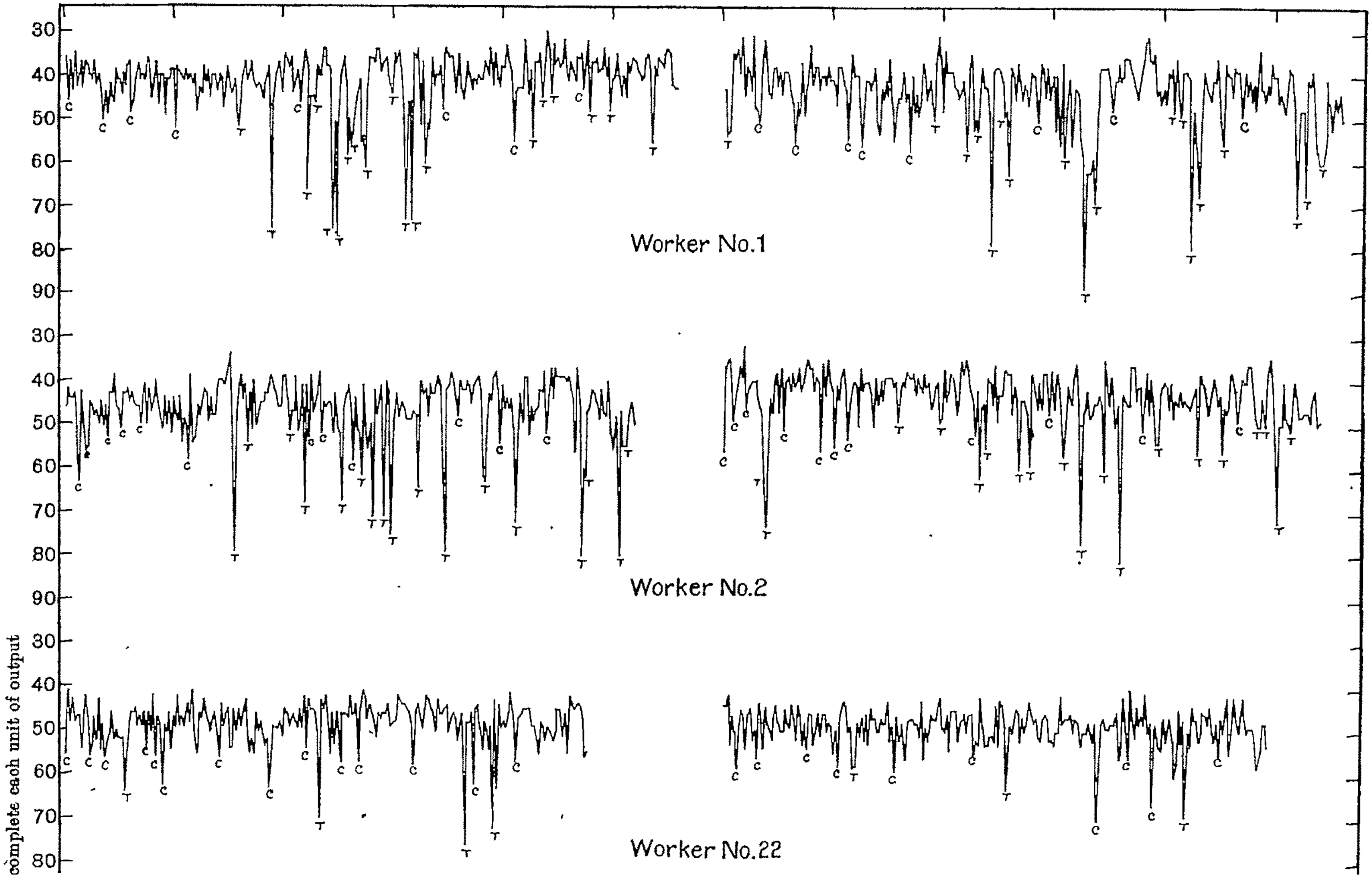
A comparison of the individual results obtained in this test with working efficiency discloses a rather interesting point. In Fig. 2 the individual scores in the test are represented as a percentage of the average for the group (broken line), and a similar procedure has been applied to the efficiency of each worker in the industrial operation (continuous line).

The curves show that the four most efficient operatives in the industrial process fail rather badly in the wrapping test, but the results of the remainder disclose a much closer resemblance between the position in the test and working efficiency. The comparative failure of the four best workers in the wrapping test may be due to temperamental causes, since they appeared to be more nervous than the others during the test performance. Whatever the cause, the indication that the best workers may be most affected by test conditions is rather disturbing from the standpoint of vocational selection.

(*c*) *Intelligence, working efficiency, and boredom.* It has sometimes been asserted that the more intelligent operatives find repetition work distasteful and boring. To obtain information on this point each worker was questioned at different times, and in particular was asked if she found the work boring or tedious at any time during the day. The replies obtained, together with the intelligence and efficiency ratings, are given in the following table.

TABLE III. *Relation between Intelligence, Efficiency, and opinions regarding Boredom.*

Worker	Intelligence	Efficiency	Replies
1	205·9	117·2	Sometimes the work gets very tedious but I know that I have to work till 5 o'clock and settle down to do so. I have to stop work to talk but like the change of company obtained by working in a different position each day.
2	192·0	117·0	Sometimes the work is monotonous and tiring to the shoulders and back. The conditions do not allow one to be very sociable. In the old method of work we could talk more and got to know each other better. Now we usually have to stop work to talk.
3	157·1	86·0	I sometimes feel bored about 3 p.m. but it improves later. Dislike sitting all the time.
4	151·8	96·1	Sometimes, about the middle of the spell.
5	143·6	101·1	The work is often boring about the middle of the spell. Often I should like to walk about.
6	143·1	91·9	I like the work very much and don't object to sitting all day.
7	122·2	87·5	The work is most tedious about the middle of the spell, but afterwards usually disappears. I get rather tired of sitting.


complete each unit of output
30
40
50
60
70
80
90
Worker No.1
Worker No.2
Worker No.22
C
T

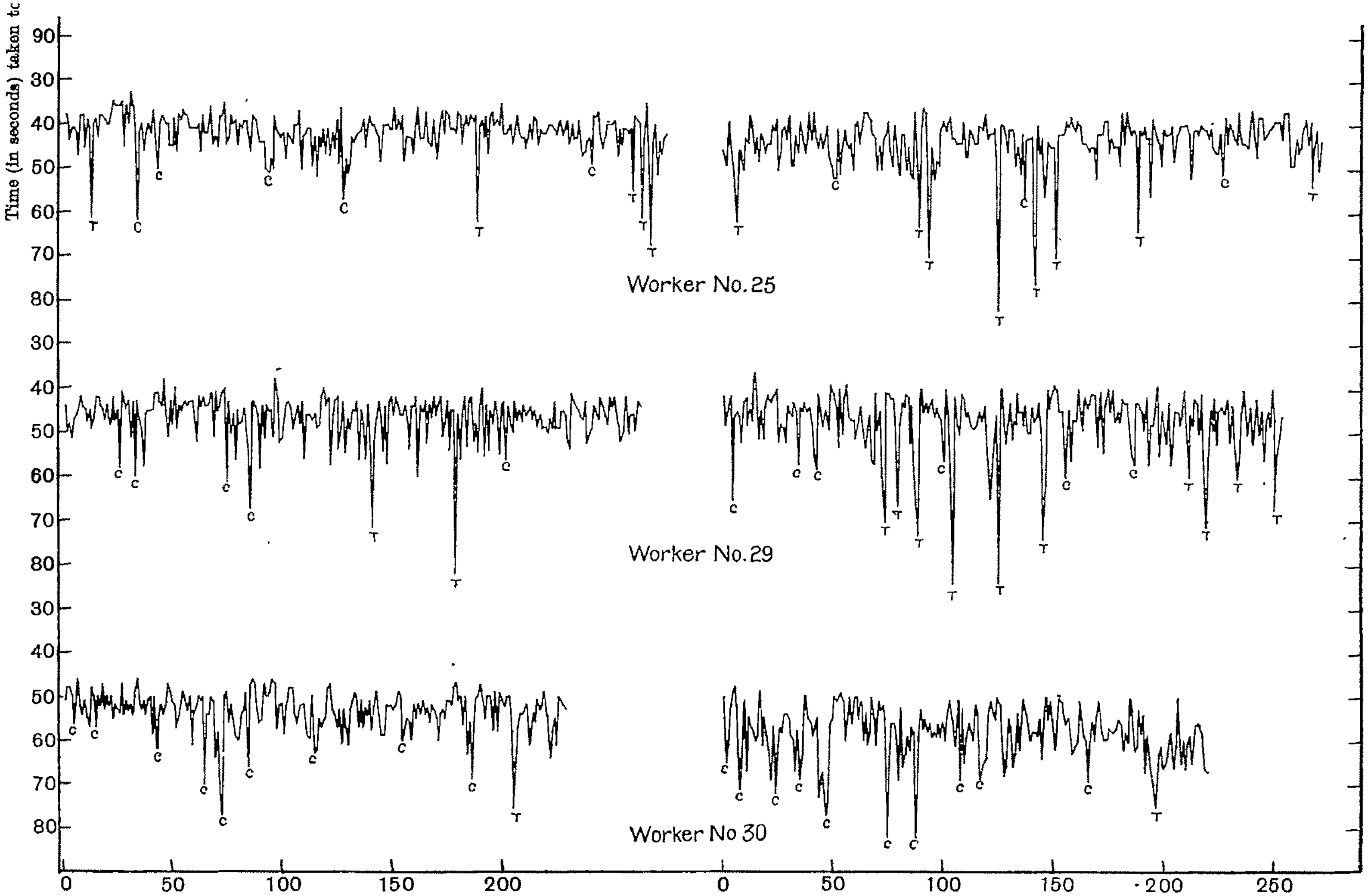


Fig. 3. Individual variations in the rate of working throughout the day.

Worker	Intelligence	Efficiency	Replies
8	118·7	99·0	When I feel bored it is usually about half-way through the morning or afternoon. Afterwards it improves.
9	116·9	93·8	Sometimes I find the work tedious about 3·30 p.m. It is nice to change places. Sometimes I should like to walk about.
10	115·2	109·7	I feel bored about 11 a.m. and 3 p.m.
11	109·9	97·5	Towards the end of the morning and afternoon the work begins to get tiring and tedious.
12	104·7	98·2	I sometimes get rather fed up about 3·30 p.m. but afterwards it gets better.
13	104·7	96·3	I don't dislike the work and seldom feel bored but I should like some exercise.
14	94·2	104·0	I enjoy the work and seldom find it tedious. Sometimes I should like to walk about.
15	92·5	95·2	I must work hard and attend to the work, I haven't much time for thinking. Sometimes I get a little weary about the middle of the spell.
16	90·8	94·8	I never feel bored and don't mind sitting all day.
17	90·8	93·8	Sometimes I find it tiring about 3 to 4 p.m. but I try to work it off. I like to do the same kind of work all the time but would prefer to walk about occasionally.
18	89·0	108·6	I feel rather weary about 4 p.m. There is too much sitting in this work.
19	89·0	109·0	Between 3 and 4 p.m. I sometimes begin to find the work tedious.
20	82·0	110·9	About 3 p.m. I find the work rather boring but afterwards it improves. I should like a change of posture.
21	76·8	112·0	I sometimes feel bored in the morning but seldom in the afternoon. The feeling disappears after dinner.
22	76·8	98·4	About the middle of the afternoon I find it tiring. Sometimes I should like to talk but find it difficult because of machinery noises.
23	68·1	113·4	I rather like doing the same kind of work all day and only begin to feel tired about 4 p.m.
24	59·3	89·3	Towards the end of the spell I begin to look around. Dislike sitting all day.
25	57·6	112·4	About the middle of the afternoon I begin to feel like a change.
26	57·6	91·3	I very seldom feel bored except occasionally towards the end of the afternoon.
27	57·6	99·6	The work gets rather tedious about the middle of the morning and afternoon.
28	52·4	92·1	I get very tired about 3 p.m. and it goes worse. Should like to walk about for a change.
29	52·4	99·2	I never find the work boring or tedious.
30	29·7	86·2	The work was monotonous at first but I enjoy it now.

Although personal opinions do not constitute scientific evidence and are seldom amenable to quantitative treatment, they nevertheless give some indication of the attitude and feelings of the workers in relation to an industrial task. The results given in the table show that 16 workers confess to experiencing some degree of boredom, and in only two cases was its presence definitely denied. Of the former, ten were above and six below the average of the group in intelligence, while in the latter, both were below the group average. It appears, therefore, that the experience of boredom, as shown by the opinions of the workers under consideration, is positively related to their degree of intelligence.

It may, of course, be justifiably claimed that the operatives were unable to distinguish between boredom and fatigue. An attempt to overcome this difficulty was made by explaining to the workers that boredom is usually shown by a loss of interest in work, wanting to do something else such as talking, looking about, or stopping work, and is rather different from fatigue which would be shown by tiredness in the fingers, arms, body, or head. Several of the replies undoubtedly denote the existence of fatigue rather than boredom, but the latter state is probably truly described when the effects are said to decrease or disappear as the end of the spell is approached.

The reaction towards the effects of uniformity imposed by the conditions of work was expressed in the behaviour of the operatives in several ways. Many of the workers complained of the discomfort produced by the continued sitting posture, and would have liked to vary the position by standing or walking about. Some found relief in the change of company produced by moving from one position to another after each spell of work. Increased conversation between the workers, especially about the middle of the spell, was also symptomatic of the desire to introduce variety into the routine procedure.

Another, and probably a more reliable method of obtaining information upon the subjective effects of repetitive processes, is furnished by detailed observations on the rate of working and behaviour of the operatives throughout the day. In the course of this investigation, six operatives (nos. 1, 2, 22, 25, 29, and 30) were each observed for four days, and the time taken to complete each cycle of movements, which included wrapping and packing three tablets of soap in a box, was noted continuously. In addition, the nature and duration of all stoppages and interruptions to activity, together with any conversations which occurred were also recorded. A typical daily output curve obtained from each worker is shown in Fig. 3. Readings accompanied by conversation are denoted by the letter 'T,' while 'C' means that the cycle of movements also includes collecting several tablets of soap from the conveyer.

The curves show that, in general, the rate of working when unaffected by collecting soap or talking remains fairly uniform throughout the day. When talking occurs, however, the rate of working is usually considerably prolonged. It will be seen that the most talkative operatives are 1 and 2;

these are also the most intelligent. Their rate of working is also the most variable[1], as the following table shows:

TABLE IV. *Showing relation between Intelligence, Efficiency, and Variability in the rate of working in the case of six workers.*

Worker	Intelligence	Efficiency	Coefficient of variation*
1	205·9	117·2	7·05
2	192·0	117·0	5·11
22	76·8	98·4	4·64
25	57·6	112·4	4·10
29	52·4	99·2	4·29
30	29·7	86·2	4·47

* The coefficient of variation is a measure of the relative variation of the observations about their mean. It is the standard deviation expressed as a percentage of the mean.

Thus, in so far as variability in the rate of working is an indication of the existence of boredom, the above results suggest that the more intelligent workers are most affected by boredom arising out of repetitive work.

(*d*) *Adaptation to monotonous conditions of work.* When the preceding results were obtained, the 30 operatives included in these experiments had been employed in wrapping and packing soap from the conveyer for a period of six months, but prior to this had been engaged for several years in similar but more varied activities. In the older process, the worker first went to the stores for wrappers and 144 tablets of soap; 72 of these tablets were then wrapped in waxed paper and afterwards in an outer wrapper. The ends of the latter were then gummed and fastened and the completed tablets packed into a small cardboard box (three tablets in each box). The remaining 72 tablets were then dealt with in a similar manner. Up to this point the operative worked in a sitting position, but now she stood at the bench and made up the cardboard boxes containing the tablets into brown paper packages. These were sealed and labelled, placed in a wooden case, and carried to a conveyer some distance away. The complete process of wrapping, packing, and dispatching 144 tablets of soap occupied approximately an hour, and involved several changes in the form of activity and posture. The conditions of work also provided many opportunities for conversation, which were freely utilized by the operatives in question. In general, the conditions appeared to be decidedly unfavourable to the onset of boredom, and the girls undoubtedly seemed to enjoy the work.

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[1] This is in agreement with the results obtained by Miss I. Burnett (Report No. 30 of the Industrial Fatigue Research Board).

When the girls were afterwards moved to the conveyer with its associated uniformity in posture and activity, signs of discontent and dissatisfaction were immediately evident. A deputation interviewed the manager and complained of the severe strain and tedium arising out of the new conditions of work. They were only persuaded to continue after much difficulty. At this stage the workers were questioned individually by the investigator, and the dissatisfaction was expressed in unmistakeable terms. The uniformity of the conveyer work appeared to be particularly objectionable, and the workers spoke rather bitterly of the boredom and fatigue it produced.

When the workers were again questioned after an interval of two months, a notable change in attitude was at once evident. The general conditions of work now appeared to be much improved, and the activities less tedious and fatiguing. Some of the workers now expressed a distinct preference for the conveyer work, and shyly confessed that they had been rather hasty in expressing their former views.

Further questioning after another period of two months elicited the fact that the workers were still more reconciled to the modified conditions, and 15 girls now stated that they would not like to return to the old method. The views of the girls obtained at this stage of the investigation are given in Table V. They were merely asked to state which method they preferred, and to give reasons for their choice (assuming in either case the same guaranteed wage at the end of the week).

TABLE V. *Opinions of the workers regarding the new and old methods of work.*

Worker	Replies
1	New. No running about. Didn't like the heavy carrying in the old. I like moving up and the change of company, but usually have to stop work to talk.
2	Old. Could enjoy the work more because of social advantages. We got to know each other better but now we have to stop work to talk because of the machinery noises. Didn't like the carrying in the old method.
3	Old. More variety and not so monotonous.
4	New. No disturbances and more encouragement to work.
5	Old. Dislike machinery and am afraid of it.
6	New. Used to waste a lot of time in the old method waiting for work.
7	New. Was often annoyed by having to wait for work. Get tired of sitting in the new method.
8	New. Can settle down. Know what I have to do and what is to follow.
9	New. No heavy lifting. Nice to change places, not so boring as sitting in one place. Used to like walking about and standing up.
10	New. Used to waste much time walking about.
11	Old. Too much of a rush in the new method.
12	Old. Can't talk now. Dislike changing places. Don't like machinery. Would rather be able to walk about.
13	Old. Not so tiring and liked the exercise.
14	New. No running about. Often had to wait, which was annoying and sometimes led to quarrels. Sometimes liked walking about as a change and miss it now.

Worker	Replies
15	New. Makes me work harder and compels attention to work. Each girl also gets the same share of work.
16	New. More certain of work. Used to like the walking sometimes but not the carrying.
17	New. All one thing and not so much lifting.
18	Old. Not so tiring and not so much sitting.
19	Old. More comfortable. More change and exercise. Not so tiring.
20	Old. Liked to walk about and to do all the different processes.
21	New. Prefer to be seated the whole time (an unusually fat girl). Now always get an equal share of work.
22	Old. Now all one thing, but used to do different kinds. Easier on conveyer because there is no carrying. Machine gets on my nerves.
23	New. More of one thing and can get settled down. Used to like walking about for a change. Would have given anything to go back to the old method at first.
24	New. Can settle down and know what I have to do and what is to follow.
25	Old. Nerves not so bad. Dislike machinery.
26	Old. Not so noisy and liked the exercise. Prefer to have things brought as it saves waiting.
27	Either. Disliked new method at first but have got used to it.
28	Old. Not so tiring or monotonous. Machine gets on nerves. Like to walk about at times.
29	New. Not so hard or heavy. Dislike lifting and carrying.
30	New. No carrying or lifting.

It is evident from the statements given in the above table that some of the girls preferred the new or more uniform method because of the existence of some particular or objectionable feature in the more varied process. One of these was the lifting and carrying of the box containing 144 tablets of soap from the seat of work to the conveyer several yards away. Six workers (nos. 1, 9, 16, 17, 29, and 30) gave this undesirable feature of the work as one of the reasons for their choice of the new method, but it was evident from subsequent conversations with the girls that they all held the same opinion.

Another factor determining the choice of the new method could be attributed to the wage incentive. In the old process it was sometimes necessary to wait for supplies of soap or boxes, and when two or more girls were waiting at the same time a certain amount of friction occasionally arose. Workers 6, 7, 14, 15, 16 and 21 gave this as a reason for their dislike of the old method, and the situation in question was undoubtedly a constantly recurring cause of irritation and discontent. Workers paid on a piece-rate basis are particularly sensitive to delays in the supply of material, and the annoyance produced by such delays often persists long after the cause has been removed.

In the case of some workers who preferred the new method, it was evident that certain aspects of the older process still appealed to them. Thus nos. 7, 9, 14, 16, and 23 liked the variations in activity and posture which characterized the old method, but at the same time appreciated the greater continuity of the conveyer work.

It appears very probable, therefore, that if the objectionable features connected with the old method, such as lifting, carrying, waiting for work, and a more equitable distribution of supplies, had been removed, almost all the workers would have preferred this arrangement, largely because of the associated variety in activity and posture. It is interesting to note that some workers (nos. 5, 12, 22, 25, and 28) appeared to be afraid of machinery and frequently remarked that "it got on their nerves." Fear or dislike of machinery is probably to be found in most groups of industrial workers, and the persistent character of such fears is a most disturbing element in their industrial life.

A general consideration of the opinions expressed by the operatives shows that different forms of activity are preferred to more uniformly repeated processes. At the same time it is evident that operatives are, within limits, able to adapt themselves to uniformity in the conditions of work. Conditions which, at the outset, may be almost intolerable because of the tedium and strain they produce, are afterwards endured without complaint and in some cases with a certain amount of enjoyment. Because of this adaptability on the part of industrial workers, repetitive processes are made more tolerable, and the existence of boredom is not nearly so prevalent or pronounced as persons unfamiliar with industrial conditions are frequently led to believe.

III. *Discussion of results.*

Perhaps the most surprising feature of the results obtained in this investigation is the low coefficients of correlation between performances in the motor tests and working efficiency. They were certainly unexpected, and fail to accord with the majority of results obtained in other investigations along similar lines[1].

The explanation of the low coefficients must be connected with one or more of the following possibilities:

(*a*) The independence of efficiency in soap wrapping upon dexterity and rapidity of hand, arm, and finger movements.

(*b*) The unsuitable nature of the tests applied.

(*c*) The failure of the tests to measure the abilities involved.

(*d*) The unfavourable conditions under which the tests were applied.

(*e*) The dependence of industrial efficiency on factors additional to manual abilities.

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[1] Cf. for instance, the reports of the National Institute of Industrial Psychology, and Report No. 16 of the Industrial Fatigue Research Board.

As regards (*a*), such an explanation is obviously absurd. Detailed timings of the operatives at work showed that an almost perfect correlation existed between rapidity of performance of each measurable component in the cycle of movements and working efficiency. It may, of course, be suggested that the more inefficient workers perform the separate movements with the same relative rapidity as the better operatives, but use a greater number of movements in the performance of the task, and consequently take longer to complete a unit of output. Such a procedure was certainly not visible to the eye, and as far as one could see the differences in efficiency were due almost entirely to individual differences in the speed of performing the same movements.

(*b*) It is possible that the tests applied fail to measure the particular form of manual dexterity involved in the industrial process. It seems difficult to believe, however, that tying knots on string, inserting matches in holes, and tapping with the finger do not require rapidity and dexterity of movement fairly similar to that involved in the actual process of soap wrapping. The nature of the tests applied, therefore, does not appear sufficient to account for the discrepancy in the results obtained.

(*c*) It is highly probable that the results obtained in this investigation fail to provide a reliable indication of the motor abilities they were intended to measure, and although the tests were repeated after an interval of three months, further applications appear to be necessary before their purpose can be achieved.

The results of psychological tests applied for the purpose of measuring particular capacities must always be accepted with a certain amount of caution and reserve. In many investigations the results of a single test are often uncritically accepted as representative of ability, yet the repeated application of the same test frequently gives rise to very different results. In the present investigation, for instance, the reliability coefficients between the first and second applications of the same tests were comparatively low, and similar results have been obtained in other investigations[1]. This means that the relative achievements of the same worker in successive applications of the same test may be very inconsistent, and consequently the results of a single test cannot usually be accepted as an accurate measure of the ability involved. Psychology, unlike the physical sciences, deals with material which may change appreciably from moment to moment, and performance during any particular test is the resultant of conditions which may be very different from those existing when the test is repeated. The acceptance of a single measurement as representative of ability is accordingly unscientific. Only when consistently high reliability coefficients are obtained can the results command conviction and respect.

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[1] See, for instance, "Motor Capacity with special reference to Vocational Guidance," by B. Muscio, this *Journal* XIII, 1922, pp. 175 *et seq.*

(*d*) It is also probable that one of the causes of the low correlations is to be found in a consideration of the conditions under which the tests were applied. The workers were wholly unfamiliar with the methods of test procedure, and were undoubtedly disturbed by the unusual situation. In many cases, signs of nervousness were distinctly noticeable, and it is unreasonable to expect an individual with trembling hands to perform efficiently a task involving accuracy and dexterity of movement. Worker No. 5, for instance, approached the scene of the tests with flushed face, quivering lips, and trembling limbs, and at the outset was unable to write her name on the test booklet. Although the experimenter attempted to soothe and calm the subject by means of suitable remarks, signs of nervousness were still apparent throughout the period of the tests. Similar, though less marked symptoms were displayed by other workers, and it was obvious that temperamental differences and disturbances were responsible for significant variations in response to the test conditions.

At the same time it should be mentioned that the general conditions under which the tests were applied were probably more favourable than such conditions usually are. The worker was comfortably seated at a desk in a quiet room, and the motor tests were preceded by the intelligence tests lasting 29 minutes, after which a short rest was taken. The experimenter was by no means a stranger to the operatives, as the latter had already been under observation for a period of three months.

(*e*) Industrial efficiency in a process such as soap wrapping usually depends upon several factors in addition to manipulative ability. Operatives may be endowed with the same amount of manual speed and dexterity, and yet differ considerably in their average working efficiency. The latter depends not only upon ability, but also upon endurance, interest, and effort expended. One operative may work steadily and consistently throughout the day. Another, equally dexterous, may show considerable fluctuations in her rate of working especially about the middle and towards the end of the spell. A third, although capable of the same rate of activity, may have little or no interest in the task, and perform the movements in a desultory manner. Motor tests, of the type considered in this investigation, would probably indicate equal abilities in the case of these workers, and would accordingly suggest that they were equally suited to the industrial occupation. The correlation between the results of the tests and working efficiency would, however, be very low, because of the overwhelming effect of differences in susceptibility to the conditions of work. It is only when the average rate of working is proportional to the maximum rate of which the workers are capable that high correlations between test results and working efficiency can be expected, since the tests usually measure maximum achievement.

The above considerations seem sufficient to explain the observed discrepancies between the results of the motor tests and industrial efficiency, and indicate the inadequacy of existing psychological tests as a means of predicting industrial achievement. The writer is forced to conclude that it is impossible to obtain an accurate indication of the essential abilities required in soap wrapping from one or two applications of the tests used in this investigation. This statement is particularly applicable to the motor tests involving speed and dexterity of movement, but is probably less true in the case of tests of general intelligence. If this belief is justified, it would appear that the present popular method of attempting to select workers on the basis of their performance in the single application of psychological tests is fraught with danger and difficulties, and must be treated with the utmost caution and reserve.

IV. *Summary of conclusions.*

The results obtained from the particular group of workers considered in this investigation show that:

1. The amount of intelligence possessed by an operative (as indicated by recognized tests) is practically no indication of her efficiency as a worker in a simple repetitive process.
2. The results of manual tests involving speed and dexterity of movement fail to give any correlation with efficiency in soap wrapping.
3. A standardized form of the actual industrial process, when presented under test conditions, failed to give any appreciable correlation with the same operation performed under ordinary working conditions.
4. The experience of boredom appears to be slightly dependent upon the degree of intelligence possessed by the operatives.
5. Variations in the rate of working (in the industrial process) are most noticeable in the case of the more intelligent operatives.
6. In general, variety in the form of activity and posture is preferred to uniformity in the industrial process.

7. Operatives are able to adapt themselves with a fair degree of success to repetitive conditions which, at the outset, may be productive of boredom and strain.

8. Dislike of an industrial process may be caused by: (*a*) having to lift and carry heavy weights; (*b*) waiting for work and the unequal distribution of supplies; (*c*) the maintenance of the same posture while at work; (*d*) machinery.

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EXPERIMENTS ON THE "DIRECTION OF ASSOCIATIONS" IN YOUNG CHICKENS

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I. *Introduction* (pp. 210–212).
II. *Description of experiments* (pp. 212–218).
  Positive learning (pp. 212, 213).
  First series of experiments (pp. 213, 214).
  Forming the connection. Onwards group (p. 214).
    Reverse group (p. 214).
    Control group (pp. 214, 215).
  Testing the strength of the connection (pp. 215, 216).
  Results of first series of experiments (pp. 216, 217).
  Second series of experiments (pp. 217, 218).
  Results of second series of experiments (p. 218).
III. *Experiments on negative learning* (pp. 219, 220).
IV. *Conclusions* (pp. 220, 221).

I. *Introduction.*

IT has been shown by Pavlov and others that when the discharge of one system of neurones A accompanies or precedes by a short interval that of another system B, then, under certain conditions, a new neurone route is opened up between A and B, so that in future the excitement of one will tend to excite the other. This new route of acquired permeability may be termed a "secondary neural connection" to distinguish it from innate or 'primary' connections. If, in the case where the discharge of A precedes that of B, the new route is such that by it A can excite B, we may say that a secondary connection has been formed in the 'onward direction'; and if it is such that by it B can excite A we may say that a secondary connection has been formed in the 'reverse direction[1].'

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[1] To avoid ambiguity the words 'onward' and 'reverse' are used here in place of Wohlgemuth's terms 'forward' and 'backward,' since the discussion in this paper deals chiefly with neural connections rather than with the related psychological associations. The use of 'forward' would be likely to lead to confusion between its meaning here and its meaning in discussions of the "law of forward conduction," the two meanings being, of course, different.

The experiments of Krestovnikov[1], in Pavlov's laboratory, show that when a sensory stimulus is given to a dog just before the salivary reflex is elicited, then, after a certain number of repetitions, a secondary neural connection will be established in the onward direction (so that in future the sensory stimulus alone will produce the salivary response); but that it is very difficult, if not impossible, to form a similar secondary connection in the reverse direction (*i.e.* that if the sensory stimulus is given just after the salivary reflex has been elicited this will produce little or no tendency for the sensory stimulus to excite the salivary response).

On the other hand, the experiments of Wohlgemuth[2] on the direction of associations in human beings have led him to the conclusion that under certain conditions (*e.g.* when a series of diagrams is learnt visually) secondary neural connections may be formed in the human cerebral cortex as easily in the reverse direction as in the onward direction. It is therefore of interest to determine the directions in which secondary neural connections may be formed in other cases besides those studied by Krestovnikov and Wohlgemuth.

The present paper describes experiments which were carried out with the object of determining whether certain secondary neural connections underlying the process of animal learning can be formed in the reverse direction as well as in the onward direction. On the psychological side the experiments are relevant to the question of the direction of associations in animals, but this paper will be confined to an account of the experiments and of the obvious physiological conclusions which may be drawn from them; further theoretical discussion being reserved for a later paper.

It is well known that if an animal receives a sensory stimulus which attracts its attention and shortly afterwards finds food in a certain place, and this whole process is repeated again and again, then in time that kind of sensory stimulation will tend to make the animal go immediately to that place. This seems to imply that a secondary neural connection has been formed (in the onward direction) between the neurones stimulated by the sensory stimulus and the very complex system of neurones which underly the actions of going to the place where food has been found[3]. In the experiments to be described it was found that with chickens about ten days old, using the sound of a horn as the sensory stimulus, such a secondary connection can easily be formed in the onward direction. In order to test whether a similar connection can be formed in the reverse direction the sensory stimulus was given *after* a chick had found food in a certain place and (after a number of repetitions of this process) the chick's tendency to go to the place at the sound of the horn was compared with that of other chicks in which the connection had been formed 'onwards' and with that of 'control' chicks in which no such connection had been established.

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[1] Krestovnikov, quoted by Beritoff, *Brain*, 1924, XLVII, p. 109.

[2] Wohlgemuth, This *Journal*, 1913, v, p. 447.

[3] A fuller discussion of the neural processes in this kind of learning will, it is hoped, be given in a later paper. For an account of the neural processes in a closely related case—that of a chick learning to peck at maggots—see Lloyd Morgan, *Life, Mind, and Spirit*, Lecture IV.

An attempt was also made to extend the results to the case of 'negative' learning (*i.e.* to the case where an 'avoiding reaction' rather than a 'seeking reaction' is learnt).

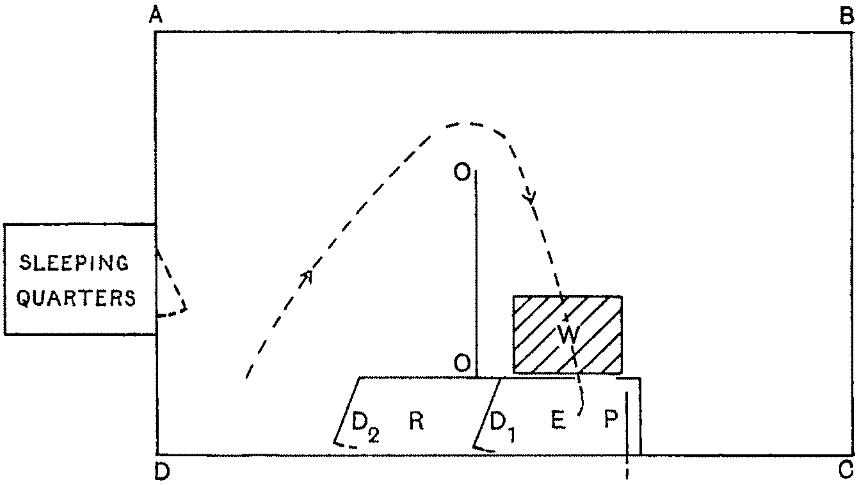

Fig. 1.

II. *Description of experiments.*

Positive learning.

The experiments were carried out in an enclosure (*ABCD* in Fig. 1), about 5 ft. × 3 ft. The experimenter, from whom the chicks customarily received food, always sat near the corner *D*; and partly for this reason, and partly because its sleeping quarters were near there, a chick placed in the enclosure usually spent most of its time near this corner. The value of this will be seen later.

The food used in the experiments was soft boiled rice. A grain of rice was placed in the space marked *R*, and to obtain it a chick starting from *D* had to go round the obstacle *OO* into the space marked *E* (climbing over the obstacle W, which was about 1 in. high). When the chick reached E the door D_1 was opened, and a piece of wood P was moved by the experimenter so that it pushed the chick into the space R. Here it found the rice, and when this had been eaten the door D_2 was opened and the chick pushed out again to the starting point. The doors D_1 and D_2 were then immediately shut—actually the doors were so made that the motion of P opened and shut them at the appropriate instants. The time between the chick's arrival at E and its ejection through D_2 was usually a little over 5 sec. The reasons for making the feeding arrangements so complex will be seen later.

First series of experiments.

Twelve chicks, distinguishable by numbered rings, were placed in the enclosure, and except when experiments were actually in progress (or when they were shut up in their sleeping quarters) they were allowed to roam about freely. During the experiments only one chick was in the enclosure at a time. The experiments began when the chicks were about a week old and continued until they were about a fortnight old.

The chicks were first of all taught to go round to E when the experimenter tapped with his fingers on a piece of wood near E; chicks which came round to E while the tapping was in progress being fed at R (as explained above), while chicks which came while the experimenter was not tapping were not fed. Care was taken that all chicks received, as nearly as possible, equal amounts of training in this habit.

When the training had been continued, at intervals, for about three days a test was made of the readiness with which each chick responded to the tapping. A single chick was placed in the enclosure and when it was walking about somewhere near D the experimenter commenced tapping and continued to do so until the chick jumped down off W into E, the time of response (*i.e.* the time taken by the chick to reach E) being measured with a stopwatch. The chick was fed at R in the usual way and pushed out, and then allowed to wander about for an interval which was usually between one and two minutes. Then, when the chick was again near D, the test was repeated, each chick being given a total of five such tests. For each test the chick was given a score equal to the reciprocal of the time of response multiplied by 100—this seemed to be a reasonable way of scoring as it gives most weight to the quick responses—and the total score for each chick was found by adding the five separate scores together. The results for the 12 chicks, arranged in order of merit, are given in Table I.

TABLE I. *Division of Chicks into Groups—Test of Response to Signal* (*Tapping*).

No. of chick (the numbers were given after the test)	Times of response (in five trials, in seconds)					Total score (*i.e.* sum of reciprocals of times)
O 1	4·8	3·7	4·8	9·0	3·8	106
C 1	4·0	6·0	4·8	6·0	5·0	99
R 1	3·5	8·5	5·2	6·8	4·2	98
R 2	7·2	5·0	4·5	4·8	5·2	96
C 2	8·0	9·0	8·0	7·5	9·0	60·5
O 2	15·0	13·0	6·8	7·2	11·0	52
O 3	15·0	11·4	20·0	6·0	7·0	51
C 3	10·0	8·0	9·5	12·5	15·0	47
R 3	14·5	30·0	10·0	9·0	8·0	44
R 4	18·0	11·0	25·0	11·0	10·5	37
C 4	10·0	14·0	12·0	62·0	12·2	35
O 4	13·0	26·0	13·0	22·0	F	24

O, C and R mean respectively 'Onwards,' 'Control,' and 'Reverse.'
F='Failed' (owing, in this case, to 'loneliness').

Total scores of the three groups:
Onwards, 233; Reverse, 274; Control, 242.

The chicks were then divided into three groups of approximately equal total 'intelligence' as measured by this test. These will be termed the 'Onwards group' (chicks O 1, O 2, O 3, and O 4; total score 233), the 'Reverse group' (chicks R 1, R 2, R 3, and R 4; total score 274) and the 'Control group' (chicks C 1, C 2, C 3, and C 4; total score 242). The three groups were treated in the following different ways.

Forming the connection.

Onwards group. With each chick the procedure of the above test was repeated 30 times in all, and in each case the horn was blown (a single blast) about 2 sec. before the tapping commenced. The sequence experienced by each chick was:

Horn—Tapping—Running round—Food.

It was noticed that when this had been repeated a certain number of times the chicks would often start off towards *E* on the sound of the horn, without waiting for the tapping.

Reverse group. In the Reverse group the procedure was the same except that the horn was blown about 2 sec. after the chick had finished eating and had been ejected through D_2. There were again 30 repetitions with each chick. The sequence experienced by each chick was thus:

Tapping—Running round—Food—Horn.

Control group. In this group the procedure was again the same, but the horn was omitted altogether.

The sequence experienced was thus:

Tapping—Running round—Food.

The interval between successive tests with each chick was about 1 min., and the chicks were dealt with on each day in random order. While one chick was being experimented upon the others were shut up in a dark box, where, however, they could still hear the sound of the horn.

Testing the strength of the connection.

This test was the same for the chicks of all groups. A chick was placed in the enclosure and when it was near *D* the horn was blown (without tapping) and the chick's time to reach *E* was noted. Scores were given as before. The test was repeated three times for each chick on two days. No food was given when the horn was blown alone, and in order that the habit previously acquired should not die out the procedure of 'forming the connection' appropriate to each group was carried out alternately with the procedure of testing the strength of the association. Details of these tests are given in Tables II and III. The numbers in brackets refer to the procedure of forming the connection and are, of course, not taken into account in reckoning the scores.

TABLE II. *First Test of Associations with Horn.*

No. of chick	Times of response to horn (the numbers in brackets show the times of response to tapping)						Total score (with horn only)
O 1	(3·0)	29·0	(1·8)	4·5	(3·7)	8·0	38·1
O 2	(2·0)	6·8	(4·0)	F	(5·8)	7·2	28·5
O 3	(5·0)	4·2	(4·5)	4·8	(3·5)	6·8	59·3
O 4	(4·2)	3·8	(3·2)	4·0	(4·8)	4·8	72·1
							198·0
R 1	(3·2)	6·2	(3·2)	15·2	(2·5)	F	22·7
R 2*	(4·7)	11·0	(3·0)	6·2	(4·5)	6·0	41·0
R 3	(6·2)	F	(8·2)	F	(6·8)	F	0·0
R 4	(7·8)	F	(6·5)	F	(23·0)	24·5	4·1
							67·8
C 1	(9·2)	29	(4·5)	11·2	(6·1)	7·0	26·4
C 2	(5·8)	F	(13·0)	F	(5·5)	F	0
C 3	(45·0)	F	(9·8)	F	(53·0)	F	0
C 4	(6·1)	F	(14·8)	F	(6·2)	F	0
							26·4

F = 'Failed,' i.e. no response in 30 sec.

* This chick ran round on the average once every 10 sec. without the horn or tapping. None of the others showed nearly such a strong tendency to do so. The 'Reverse' score is probably too high therefore.

TABLE III. *Second Test of Associations with Horn.*

No. of chick	Times of response						Total score
O 1	(1·2)	4·8	(4·0)	3·2	(4·0)	5·0	72·0
O 2	(2·5)	20·0	(4·8)	F	(2·5)	26·0	8·8
O 3	(11·0)	8·0	(6·8)	9·0	(5·8)	14·2	30·7
O 4	(3·5)	17·0	(2·8)	6·1	(2·8)	6·5	37·6
							149·1
R 1	(4·1)	F	(3·8)	F	(3·5)	F	0·0
R 2*	(6·0)	5·0	(5·6)	18·0	(2·8)	F	25·5
R 3	(8·0)	25·0	(7·0)	F	(5·5)	8·0	16·5
R 4	(13·5)	29·0	(8·2)	F	(42·0)	F	3·4
							45·4
C 1	(7·5)	6·2	(4·7)	15·0	(2·8)	F	22·9
C 2	(12·2)	F	(6·2)	17·0	(9·2)	28	9·3
C 3	(9·2)	F	(7·9)	F	(150·0)	F	0·0
C 4	(8·0)	F	(7·2)	F	(9·5)	F	0·0
							32·2

F = 'Failed' (in 30 sec.).
Interval = about 1 min. in all tests.

* This chick again showed a strong tendency to run round without the horn or tapping. The 'Reverse' score is, therefore, probably again too high.

Results of first series of experiments.

A summary of the results is given in Table IV. The total scores for the three groups are: Onwards 347, Reverse 112, Control 58. The difference between the Onwards and Control scores is equal to about five times the sum of the probable errors. We are, therefore, justified in concluding that a secondary connection has been formed in the onwards direction.

The difference between the Reverse and Control scores is much less; it is only a little greater than the sum of the probable errors, and this difference is due entirely to the high scores obtained by one of the Reverse chicks, R 2, which had developed a habit of running round *E* at frequent

TABLE IV. *First Series of Experiments. Summary of Results.*

	Onwards group	Reverse group	Control group
Score in first test	198	68	26
Score in second test	149	44	32
Total score	347	112	58
Probable errors in total scores (calculated from total scores of each chick in each test)	±40	±28	±21
Responses in less than 10 sec. (24 possible)	17	5	2
Responses in less than 5 sec.	9	1	0
Failures	2	13	17

intervals whether the experimenter gave any signal or not. It is, therefore, not possible to decide from these results whether a reverse connection has or has not been formed. All that we may conclude is that if a reverse connection has been formed at all, it is a great deal weaker than the corresponding onwards connection. The difference is all the more striking when it is remembered that the interval between the horn and the food is only about 3 sec. in forming the reverse connection, while it is nearly always 6 sec. or more in forming the connection in the onwards direction.

Second series of experiments.

In a second series of experiments an attempt was made to correct for the chicks' tendency to go round to E 'spontaneously,' in order to find whether the difference between the Reverse and Control scores in the previous experiments was really significant.

Only two groups of chicks, Reverse and Control, were used. There were five chicks in each group and the groups were of approximately equal total 'intelligence' (measured as described above). The chicks were of about the same age as those used in the previous series (*i.e.* about 5 days at the beginning and 12 days at the end of the experiments).

The process of 'forming the connection' was carried out almost exactly as before, but one alteration was introduced to meet the possible objection that the difference between the Reverse and Control scores might be due to a weak onwards connection between the neurones involved in hearing the sound of the horn and the finding of food in the *next* trial, about a minute later. To meet this objection the horn was blown after each trial both with the Reverse and with the Control chicks; but while with the Reverse chicks it was blown, as before, about 2 sec. after the ejection of the chick through D_2, with the Control chicks it was blown about half a minute later, and the interval between trials was made correspondingly longer with the Control chicks. Under these conditions we might expect any such 'long' onwards connection to be of about the same strength in the two groups.

The correction for each chick's tendency to go to E 'spontaneously' was made in the following way. Alternately with the measurement of the time taken by the chick to go to E when the horn was blown, the chick's time to go to E with no signal was measured. This latter measurement was made in each case from some arbitrary time when the chick was standing near D; about a minute after its ejection through D_2, *i.e.* from a moment which would have been suitable for blowing the horn.

Scores were given for these responses in the usual way, and for each chick the scores so obtained were subtracted from those obtained as before.

During these tests the horn was blown once every 5 sec. (up to half a minute if no response occurred earlier) instead of only once as in the previous experiments.

Results of second series of experiments.

Details of the test for Reversed connection are given in Table V (where the numbers in the first column refer to 'practice' trials, cf. p. 218). The total 'corrected' scores are

Reverse ... 23 (probable error.15)
Control ... 25 (probable error 6)

TABLE V. *Result of Second Series of Experiments.*

No. of chick	Times of response in 'forming'					Times of response to horn					Total score with horn	Times of response without horn					Total score without horn	Total 'corrected' score
(*Reverse group*)																		
1	14	9	6·5	4·8	6	20	6·5	10	12·5	11	47	F	12	8·5	16	F	26	21
R 2	5	6	4	5·2	4·7	F	10	10	8	6	34	16	11	10	7	F	40	−6
R 3	12	8	8	6	11	F	F	F	10	F	10	F	F	F	F	17	5·8	4
R 4	4·8	3·8	5·5	7	3·8	17	8	F	F	6·5	33	F	13	F	F	6	24	9
R 5	12	9	4·2	3·8	4·7	F	F	F	F	F	0	F	20	F	F	F	5	−5
(*Control group*)																		
C 1	15	9	6·2	12	15	F	4·8	10	7	F	44	10	9·2	F	9·8	24	35	9
C 2	15	5	6	4	6·2	11	20	11	F	14	30	F	16	F	12	15	21	9
C 3	4·6	3	8·8	5·5	F	6·6	29	4	F	F	43	4·5	6·8	F	F	F	37	6
C 4	16·5	7	4·8	12	9	F	12	F	F	F	8	F	F	F	27	19	9	−1
C 5	22	7	20	10	14	F	F	F	F	9	11	24	20	F	F	F	9	2

Total 'Reverse' score . . +23 (probable error ±15).
Total 'Control' score ... +25 (probable error ± 8).

(If an Onwards group had been trained in the same way we should expect from it a score of about 300, since the conditions are almost the same as in the first series of experiments.) The very close agreement between the two scores shows that no detectable Reversed connection had been formed.

The fact that both groups show positive scores which are greater than the probable errors suggests that the sound of the horn produces a weak tendency to go to E; possibly owing to some slight similarity between the sound of the horn and that of the tapping, or possibly because the sound of the horn increases the general activity of the chicks, or possibly by a 'long' onwards connection. This point was not investigated further.

III. *Experiments on negative learning.*

The method which was first adopted in the experiments on negative learning depends on the fact that rice soaked in a solution of quassia extract is 'nasty.' A number of chicks were given rice which was sometimes nice and sometimes nasty (*i.e.* sometimes soaked in water and sometimes soaked in quassia). The chicks were divided into three equal groups, as before, and with those in the 'forward' group the horn was sounded before giving nasty rice, while with those in the Reverse group the horn was sounded after nasty rice had been given. Before the training was sufficiently far advanced for any tests to be made it was, however, found that nearly all the chicks either would not peck at all at rice or else would swallow the rice (whether soaked in quassia or not) with little sign of dislike. An attempt was then made to carry out an experiment on the same principle substituting an electric shock for the taste of quassia; the rice being placed on an insulated table to which a voltage could be applied at will so that a chick, on pecking at the rice, received a sharp electric shock. The same difficulty was met with here as in the previous case.

It was, however, found possible, using the sound of the horn and the reaction to an electric shock, to examine the case of secondary connections of the ordinary 'conditioned reflex' type; the experiment being merely an extension of Krestovnikov's work to the case of a reaction to an unpleasant stimulus. The chicks were placed, in turn, in a box about 1 ft. square, the floor of which was covered with a system of parallel wires about ½ in. apart. Alternate wires were connected to opposite poles of a battery of dry cells and, by means of a switch, it was possible to give the chick a shock through its feet. On receiving a shock a chick would usually jump into the air, and chirp. Sometimes it would also shake its head vigorously or flap its wings.

The chicks were divided into three groups as before, there being six chicks in each group. Each chick was given a total of 20 shocks, at irregular intervals, the horn being blown about 2 sec. before each shock for the chicks in the Onwards group and about 2 sec. after each shock for those in the Reverse group. The control chicks heard the horn the same number of times while in the experimental box, but in their case there was no relation between the times of the horn and the shock.

After this training each chick was again placed in the experimental box, and the horn was blown six times (at irregular intervals) and the chick's reactions were noted. These tests were repeated on the following day. (As in the previous experiments the procedure of forming the connection was carried out alternately with that of testing the strength of the connection in each case.)

The results are given in Table VI. It will be seen that with the chicks of the Onwards group the sound of the horn usually produced an appreciable movement; most frequently a sudden turning or shake of the head, sometimes a sudden turning of the whole body and sometimes an actual jump into the air. With the chicks of the Reverse and Control groups, on the other hand, head movements occurred far less frequently, and practically no jumps or turns were observed. There was no significant difference between the observed behaviour of the latter two groups.

TABLE VI. *Results for Negative Learning. Responses to Sound of Horn in* 12 *Tests.*

No. of chick (O = Onwards R = Reverse C = Control)	Jumps into air	Turns or other sudden movements of whole body	Head movements
O 1	1	5	3
O 2	4	—	1
O 3	1	—	8
O 4	1	4	7
O 5	1	4	4
O 6	2	3	3
R 1	—	—	3
R 2	—	—	4*
R 3	1	—	5
R 4	—	1	1*
R 5	—	—	1*
R 6	—	—	1
C 1	1	—	5
C 2	—	—	4
C 3	—	—	—
C 4	—	—	3*
C 5	—	—	—
C 6	—	—	5

* These cases were doubtful, the chick moving continually without reference to the horn.

Summary.

	Onwards group	Reverse group	Control group
Total no. of movements ...	38	14	14
Total no. of movements (omitting doubtful cases)	38	8	12
Turns, etc.	16	1	0
Jumps	10	1	1

IV. *Conclusions.*

The experiments described in this paper afford evidence that in chickens about ten days old a secondary neural connection may readily be formed in the 'onward direction' between the neurones stimulated by the sound of a horn and those underlying the tendency to go to a place where food has been found, but they give no evidence of the formation of similar connections in the 'reverse direction.' If 'reverse' connections are formed at all under the conditions of these experiments they are very much weaker than the corresponding 'onward' connections. The same conclusion applies to connections between the neurones stimulated by the sound of a horn and some of the motor neurones involved in the reaction to an electric shock. These results are in agreement with those of Krestovnikov in the case of the salivary response in dogs.

On the psychological side the results suggest that certain of the associations formed in an animal's mind during the process of learning are similar to Wohlgemuth's 'motor associations' in human beings in that they are only formed in the 'forward direction.'

I wish to express my thanks to Professor Lloyd Morgan for his interest and advice, and to Professor A. P. Chattock, of the Department of Physics, for his kind help with the experimental work.

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A BRIEF THEORY OF THE ORGANISM SUGGESTED BY AN EXPERIMENT ON THE PERCEPTION OF ALMOST INAUDIBLE SOUNDS

By R. W. PICKFORD.

(*From the Cambridge Psychological Laboratory.*)

I. *Objects of the experiment* (pp. 222, 223).
II. *Experimental procedure* (pp. 223, 224)
  (*a*) *Apparatus* (p. 223).
  (*b*) *Arrangement of experiment* (pp. 223, 224).
III. *Results* (pp. 224–229).
  (*a*) *Results from conditions experimentally modifiable* (pp. 225, 226).
  (*b*) *Results from general 'non-experimental' conditions* (pp. 226–229).
    Sleepiness (p. 227).
    Affect (p. 227).
    Emotion (p. 227).
    Imaging (pp. 228, 229).
    Attention (p. 229).
    Auto-suggestion (p. 229).
IV. *General theory* (pp. 229–233).
  Conclusion (p. 234).

I. *Objects of the experiment.*

It has frequently been suggested that when sensory impressions of differing mode[1] are simultaneously presented one set may, under certain conditions, facilitate or hinder another. There is for example a widespread popular belief that auditory perception may be helped by the simultaneous presence of certain colours, and rendered more difficult by the presence of others. The first object of this experiment was to test such a theory.

If a subject is at the same time and under carefully controlled conditions supplied with fully liminal colour stimulation, and sub-liminal auditory stimulation, any effect of the colour upon the sound should be at once apparent. Obviously any extensive investigation of this type would demand the use of a very large number of colour-sound combinations,

[1] See Myers, *Text Book of Experimental Psychology*, Vol. I, 113.

and a wide range of subjects. It appeared probable, however, that, even if both of these were restricted greatly, it might be shown whether or not the matter is worth an extended and complete investigation. In the meantime I had a secondary object which, as the experiment proceeded, seemed to increase greatly in importance.

It is, of course, generally admitted that even the simplest perceptual processes are psychologically determined in various ways which cannot be adequately described in physical or physiological terms. Yet psychologists, when they investigate these processes, are, on the whole, far too ready to preoccupy themselves with physical and physiological conditions, as if there was no genuine *psychology* of sensory response. I therefore decided to give special attention to the conditions of reaction to barely audible sounds which may properly be described as psychological, and as the experiment proceeded I realised more and more fully that conditions of this type are of the greatest interest and importance.

II. *Experimental procedure.*

(*a*) *Apparatus.* It is not necessary to describe the apparatus used in great detail. Substantially it was much the same as that employed by Dr H. Banister in investigations already reported[1]. This apparatus was adapted so that a suitable tone (of about 384 vibrations per sec.) could be presented at sub-liminal, liminal, or fully audible intensity. All the experiments were conducted in the sound-proof room of the laboratory. Adequate control or 'catch' experiments were used throughout.

The subject sat at a small table in the sound-proof room, wearing a pair of head telephones. A three-sided tissue paper screen 18 in. square at the base and 2 ft. 6 in. high, was erected on the table. The screen was of red-orange, ultramarine, or golden yellow as desired, and a bright lamp immediately above with a sheet of tissue paper under it made a brilliant coloured area within the screen, as the tissue paper transmits selectively almost as well as coloured glass. The subject sat with his head and shoulders inserted in the screen throughout each experiment, and recorded the observations upon paper of the appropriate colour.

(*b*) *Arrangement of experiment.* Practically all sittings took place in the afternoon between 2 p.m. and 4 p.m., and lasted for about one hour. A suitable range of intensities having first been decided by actual trial, every sitting was divided into six 'series' each containing 20 individual observations. The work of an entire sitting was called a 'group,' and a component 'series' was sub-divided into two sets of ten observations each, though the observations in a 'series' were numbered throughout 1 to 20. Between each 'series' an interval of three or four minutes was allowed. Every 'series' was preceded by three flashes from a signal lamp, and a similar signal was given at the end of ten observations, and again to terminate the 'series.' This facilitated the location of observations and prevented confusion, for, if the subject lost count or missed a signal, it was possible to confine the error to a set of ten observations. The subject simply wrote down 'Yes' or 'No'—according to whether he heard or did not hear a sound—in a column of a blank form provided, putting in three ticks every time he got the signal of three flashes. At the end of each series of 20 observations the observer removed the telephones. The latter were therefore moved 11 times a sitting, and since there turned out to be only a very few different ways of wearing them compatible with reasonable comfort, these different ways must have been distributed over the entire experiment (of about two dozen sittings) in a totally irregular manner. Any possible effect from this source of error will therefore be neglected. In each set of ten observations there were five appearances of the signal flash when there was also a sound, and five when there was not. The order of these was arranged by the experimenter to be as irregular as possible, prior to the sitting, and was quite unknown to the subject. Thus in a sitting of approximately one hour the sound was actually present for only 10 mins. Having regard therefore for the low intensity of the stimulus, the question of *purely auditory* fatigue is probably quite irrelevant. At the end of each series the experimenter altered the intensity by a small, measurable step. On alternate days the series of a group increased, and decreased. The experimenter recorded intensity, pitch, colour, date, and time, of each series, and whether a stimulus sound had been provided or not. The subject recorded everything that occurred to him that he had time to write down.

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[1] This *Journal*, Vol. XVI, 1926, 269.

III. *Results.*

The adoption of experimental procedure enables the worker to create a certain number of experimentally modifiable artificial conditions. If the experimenter chooses to regard these as causes, he may, after altering them, regard any secondary changes involved as effects of the alteration. This is common to all sciences, but particularly in the organic field the worker can also enumerate a number of conditions in the erection of which he played no part, and then obtain circumstantial evidence of their causal value, by using the observations of the former class as 'controls.'

(*a*) *Results from conditions experimentally modifiable.*

If the threshold obtained in *Section A* (where no coloured paper was used) be represented by unity, then the remainder of the experiment appears as shown.

I. SUBJECT P.

Section A. (Groups I and II.)

'No' colour	Intensity Increasing	1·1
	Intensity Decreasing	0·9
	Average	1·0

Section B. (Groups III to VIII, and XVI.)

Red-orange	Intensity Increasing	0·98
	Intensity Decreasing	1·00
	Average	0·99

Section C. (Groups IX to XV.)

Blue	Intensity Increasing	0·9
	Intensity Decreasing	1·1
	Average	1·0

Section E. (Groups XIX to XXIV.)

Yellow	Intensity Increasing	1·15
	Intensity Decreasing	1·15
	Average	1·15

II. SUBJECT W.

Section D. (Groups XVII and XVIII.)

Increasing (Blue)	1·0
Decreasing (Red)	0·7
Average	0·85

No attempt will be made to interpret these results. That there *were* differences is evident; that they were in any way owing to the modification of conditions in the experiment can only be inferred with the greatest caution, with so small an amount of material to go upon. However it is worth noticing that there was an interval of five weeks between *Sections C* and *E* which may account for the change on threshold being so marked.

Subject *P* reported the red colour very irritating in the first group in which it was used, but the results do not appear to have been influenced.

The irritation was not referred to again even on returning to red in Group XVI, after blue.

Subject *W* reported no sense of annoyance at red.

Subject *P* reported blue very pleasant all through, but the results were not affected. However Subject *W*, who reported blue particularly pleasant, showed a marked lowering of threshold at the same time. It is worth noting that the actual intensity of illumination may have been lower in blue than in red.

Subject *P* rather refrained from making judgments as to whether the intensity was increasing or decreasing, but when he did this the results are of interest.

Group I. Judged increasing. Actually decreasing. Correct replies increased in frequency throughout the group.

Group X. Correct judgment.

Group XIII. Judged correctly that it was decreasing, but felt sure he was improving as the sitting progressed, though he was not.

Group XX. Tried to remember whether it should increase or not, so as to help, but failed.

Groups XXI and XXII. Correct judgments.

Group XXIV. Judged increasing. Actually decreasing. Number of correct replies increased throughout the group.

It is apparently very difficult for the subject to judge on this point, and a mistake may lead to a reversal of replies so that more correct ones are returned though the sound becomes less audible. But it might be that the accuracy changed for an unknown reason and was the basis of his judgment. The questions of the relationship between current intensity and amplitude of diaphragm-vibration; and between amplitude of aerial vibration and noise perceived, are very complex and will not be discussed here.

(*b*) *Results from general 'non-experimental' conditions.*

Under this heading will be considered the subjects' introspections, or attempts to make a critical—though not necessarily rational—account of their behaviour, and other personal details which were recorded as of interest. In order to further experimental psychology, it would be desirable for the observations of this section to be brought as much as possible under the class (*a*). The task of making a genuinely unbiased critical introspection is probably the most difficult presented in science, but the attempt to make psychology 'scientific' by eliminating all critical introspection, on *a priori* grounds, has been somewhat unfruitful, and would appear, to say the least, itself a rather unscientific proceeding. Hence a technique must be elaborated which will assist the task of critical introspection. The attempt to introspect in a justifiable manner depends upon the existence of an insight into personal motives, etc. which is not a very frequent attainment, and is unfortunately sometimes socially banned as a slight upon human nature, except where it is permitted under the protection of medicine.

Sleepiness. This occurred to a surprising degree. In a considerable number of instances the subject actually skipped one or two individual readings, producing no record of them. In more generalized cases it simply appeared as a tendency for a sudden drooping, or relaxation of postural tone, with the characteristic periodic voluntary efforts to prevent this.

In 12 out of 24 series where sleepiness was recorded, and in 37 out of 107 series where it was not, the Subject *P* returned 80 per cent. (or more) of correct replies. In *Section B* where sleepiness was most recorded the threshold was lowest, while in *Section E* where it was least recorded the threshold was highest (for Subject *P*). Subject *W* did not record sleepiness at all.

Affect. (*a*) *Pleasurable.* The only notable record of pleasurable affect was with Subject *W* in Group XVIII. The pleasurable affect was referred to the blue colour. It was described as: restfulness, enjoyment, lack of sleepiness, and ability to concentrate. The subject returned 119 correct replies out of 120. This was the highest on record for both subjects.

(*b*) *Indifferent* affect was present in the greater number of cases, but was not recorded. Where there were fewest records of affect the threshold appears to have been rather high (in *Section E*).

(*c*) *Displeasurable.* A variety of terms was used by Subject *P* to describe his affects of displeasure, such as: irritation, distraction, positive annoyance, psychological tension, and inability to control his attitude to the experiment. Where these symptoms were recorded most (in *Section B*) the threshold was lowest recorded for Subject *P* (*cf.* (*a*) above).

Emotion. This is arbitrarily distinguished from affect by the criterion that it contains a large element of organic disturbance. Or it may be used to signify the motor aspect of the same condition of which affect signifies the subjective aspect. The one emotional condition recorded was an excessive and persistent increase in the rate and strength of the heart-beats, but it may be taken that this was always accompanied by a generalized condition of motor agitation, and frequently by slight headache. The normal heart-beat is quite noticeable when the ear-phones are put on, but if this is at all increased it becomes surprisingly loud, especially in the sound-proof room. Sometimes it is possible to avoid attending to it, but most frequently the result is that the subject listens for the stimulus in the intervals between a succession of thumps. The stimulus cannot be distinguished at the instant of a thump. It is not evident from the results that emotion, in this sense, makes any serious difference.

Imaging. Under this heading will be included (*a*) Dreams, (*b*) Auditory imagery of special interest.

(*a*) *Dreams.* Few of the actual dreams were recorded as the subjects were not experienced in introspection on dreams. It is certain that Subject *P* was occupied with day-dreaming a large part of the time, and hardly ever with organized thought. When 'sleep' was recorded the subject had no conscious content. Sleep generally meant that day-dreaming ceased. Sleepiness was compatible with day-dreaming in which the subject felt an ability to introspect upon the 'part of him' occupied with the dream, but when actual dreaming occurred, as it did several times, this critical ability was lost. The conditions of the experiment did not allow of a reasonable investigation of dreams as the time-intervals were too short for any records to be made. On the most notable occasion when a dream was recorded the percentage of correct replies was a little low, but so many dreams were deliberately omitted that nothing can be inferred.

(*b*) *Special cases of auditory imagery.* The stimulus on many occasions seemed positively prolonged after the cessation of the light-signal. Only 10 per cent. of these apparent positive after-sensations coincided with the absence of a stimulus.

Images of the stimulus apparently not connected with the occurrence of the light-signal were recorded a number of times. The results were not strikingly bad when this tendency to image was in evidence.

The most frequent auditory imagery recorded was of extraneous material which perseverated in the shape of memory-images. This occurred when the subject had been practising a new piece of music on the clarinet, which interested him much. Under these conditions in *Section B*, Group VIII, the results are accurate, and in *Section E*, Group XXIV, they are strikingly inaccurate.

'Singing' in the ears will also be included for the present purposes under this head. This only became a disturbing influence in one case, when it increased every time an attempt was made to listen, otherwise disappearing. The sound-proof room serves to exclude most external noises, but only to accentuate the intrinsic hum of the cerebro-auditory apparatus. This consisted largely of low-pitched components, except for the familiar 'singing.' Any sounds which are critically perceived have to be distinguished from this, and not added to it. The subject is bound to keep perfectly still. If he makes the slightest overt movement nothing can be heard of the stimulus. Breathing contributes a good deal of noise, and if a facial or neck muscle be contracted at all strongly a roaring noise is produced.

Attention. The recording of processes of attention seems to be the most difficult part of the introspection. The records are probably unreliable because the subject tends to record inattention when he would be unable to deal with a difficult intellectual problem, which may be just the time best suited to dealing with minimal auditory stimuli. The results were unusually good where inattention was recorded. In one case where they were exceptionally bad Subject P recorded a feeling that he had to 'hold on to' the auditory perception "metaphorically, in attention, for fear of losing it before the signal switched off," which is a confirmation of the perception.

Auto-suggestion. The observations in *Section E*, Group XIX, are interesting. In the third series Subject P recorded great agitation, and gets $\frac{2}{10} R$ and $\frac{1}{10} X$. (Read: 2 out of 10 positive replies when there *was* a stimulus, and 1 out of 10 positive when there was *not.*) In the next set he 'made up his mind to hear it better,' and tried to induce a dreamy state. At number 17 he counted that he had the full complement of ten positive replies and decided 'not to hear any more.' He did not 'hear' any more. This is a deliberate occurrence of a thing that often happened unwittingly. In the first half of the next series he suggested 'I *can* hear it,' every time, and got $\frac{5}{8} R$, and $\frac{2}{8} X$. In the second half he reversed the suggestion and got $\frac{4}{8} R$, and $\frac{0}{8} X$. In the next series he suggested to himself, 'I am indifferent whether I hear it or not,' and got $\frac{7}{10} R$ and $\frac{0}{10} X$. However no conclusion can be drawn from this as the intensity was increasing at the same time, and the observations on this question were only made once.

IV. *General theory.*

The *raison d'être* of an experimental enquiry is twofold. In the first place it enables answers to be given to certain classes of question which appear prior to the elaboration of the experiment. In the second place it makes it possible to ask further questions, some of which, in turn, may be answered by further experiment. This record, therefore, would be incomplete unless some reference was made to the problems which are suggested by the experiment.

I shall present my conclusions in the form of a hypothetical reconstruction, which is regarded as tentative, as some parts of it are open to further experimental treatment and it is all open to theoretical revision.

(*a*) It is suggested that the organism may be approached from three points of view. Each of these might be the foundation of a complete mode of investigation, or they might be combined. Further it might turn out that each of them represented an *actual* scheme of Nature, or that one or more of them were only working principles. Again, as stated they will appear to form a series of increasing complexity which, taken as a whole, might represent the process of development of organic response in evolution. But even if this was so, it would still be conceivable that, at any given stage, such as the present, the earlier phases of development might not be discoverable as such. It might simply be desirable to assume that some such stages had been passed through, and later, bodily incorporated in the stages subsequent to them.

The stages, or points of view, are those of 'Response,' 'Balance,' and 'Initiation.'

(*a*) '*Response.*'

There appear to be certain cases in which, when an environmental disturbance is communicated to an organism, it accounts completely for all the conduct of the organism which occurs while it is in operation. These cases are known as Tropisms. In the majority of instances, however, the actual energy imparted to the organism is rapidly expended, but it initiates a train of internal disturbances. Numerous conditions then come into operation with the result that the ensuing train of disturbances is characterized by marked features which lead an observer to refer the train to one or other of certain arbitrary classes. It is then described as a 'response.' Generally speaking the organism is subjected to such a complicated group of stimuli simultaneously, that, if a classification of such groups shows any remarkable correspondence with a classification of responses, it is as well to call such groups 'situations.'

Whenever it is intended to support this theory of action it is necessary to demonstrate that every observable organic (or psychic) event is the result, direct or indirect, of some sort of extra-organic event. When the causation is indirect the latter must be regarded as operating *viâ* some mechanism of organic morphology, or 'traces.' We thus get various 'mnemonic' theories of the organism.

(β) '*Balance.*'

However, the organism mostly exhibits a certain number of responses which are not only externally initiated, but which are also apparently internally initiated in some measure. It is for instance difficult to give an adequate account of appetite as a factor in conduct in terms of externally initiated activity solely. In this theory of action the internal selective factors are regarded as having an autonomy, or spontaneity. The organism becomes regarded as a bundle of tendencies, or potential reactions, which contain within themselves potential conduct, so that once conduct has been initiated it is not explicable purely in terms of present, or previous, situations. Here the word 'disposition' is admissible. The external situations are only *means* of conduct, while the internal conditions are *more* than structural copies of conduct one of which must be 'expended' each time that specific conduct is elicited, as on Weissmann's theory of morphogenesis. Where this theory is to be supported therefore, it must be shown that the organism is a balancing apparatus. It contains tendencies which are autonomic with the exception that they are only functional in co-operation with specific external conditions.

(γ) '*Initiation.*'

If it was demonstrable that a further type of conduct occurred in which behaviour appeared to be independent of external conditions, or in which the organism had the power to organize the external means in accordance with the internal dispositions, as well as to re-organize 'traces' so that they were not mere copies of possible conduct, we should have to speak of the organism as an initiating apparatus. So long as the whole of conduct is activated from without it is necessary to regard the organism as a mechanism adjusted to its environment. When conduct is initiated from without and also from within, there is still nothing but adjustment, and as soon as conditions are encountered foreign to the adjustment the machinery breaks down. If this is to be avoided, it is necessary that the initiation of conduct should come from within in such a way that either present external conditions should be modified to conform with potential conduct, or that potential conduct should be modified to conform with present conditions. The latter step might involve either or both of two processes. Firstly the re-organization of mnemonic factors so that they do not operate in precisely the structure in which they were laid down; secondly the reconstruction of the systems of dispositions or tendencies which operate these 'traces.' The term 'adaptation' is properly used here.

(*b*) *Sleepiness.* It is suggested that a moderate degree of sleepiness is an advantage, in the experiments described, as it saves the subject from distraction. The subject of this experiment was called on to make a very simplified reaction to a very simplified situation. It would be an advantage, therefore, to simplify the subject to an apparatus of a very low order. Sleepiness may therefore be regarded as a discontinuance of some complications operative on ordinary conduct, and so transferring the bulk of the initiative to the external conditions.

Affect. The subject worked under the impression that psychogenic disturbances of an affective order had a great influence on the results, but from the experiment it does not appear as much so as he thought. However in view of the very small differences in threshold which were found to correspond with the very large differences of frequency of record of displeasurable affect in the case of Subject *P*, it would seem possible that either (*a*) there was some contingent oversight in the subject's record, or (*b*) that the occurrence of displeasurable affect is actually connected with a decrease in the acuteness of introspective analysis. I do not wish to pursue the latter problem further here, but in either case one is inclined to doubt the actual evidence, which is self-contradictory, and so to re-assert the finding of common sense that pleasurable affect accompanied the successful carrying out of the necessary adjustments of conduct, on the part of the subject, to the conditions enforced; while displeasurable affect accompanied the failure. The same theory seems applicable to emotion. Hence the problem of affect and emotion refers us immediately to the 'Balance' conception. That is, at least, without a much more intricate analysis (experimental and theoretical) we cannot say definitely that genuine 'adaptation,' of an initiative order, was involved, either successfully or otherwise, when affect formed part of the reaction.

Imaging. The subject worked under the impression that one of the bases of his judgments was the critical comparison of memory-images of the stimulus with the actual sound, and also that he was much better able to judge 'yes' or 'no' if he had a strong tendency to produce images. But when he did have striking images, he records confusion as the result. We might suggest that it is possible for a memory image of the stimulus to be partly activated internally when it is in a state of readiness. Then when the stimulus arises externally the perception will be rendered easier. Here we have the conception of 'Balance.' But if the stimulus does not arise, internal activation may or may not be completed. If it is, the subject records having imagined a sound. If it is not, he simply records having 'attended.' If the partial activation is of the wrong sound, or if it is completed prior to the arrival of the stimulus, confusion might result. This occurred where other memory images tended to perseverate. Here we get the conception of the 'Initiating' apparatus and a pursuit of this problem might throw a very interesting light upon the psychology of imagination and artistic interpretation.

A consideration of positive after-sensations would take us back to the view of the organism as a 'Responding' apparatus. Since the after-images were not peculiar to loud stimuli there is no suggestion that they are due to fatigue or to a mere mechanical prolongation of the effects of the stimulus-energy. Hence they may be regarded as continuations of the response disturbance initiated by the stimulus, in some part of the organism. It is interesting that both subjects passed through a phase in which they felt that the existence of an after-sensation was a confirmation that there had been a real objective stimulus.

Attention. This question seems to be closely allied to that of imaging. It seems not improbable that the attention which was referred to by the subjects was of a quite different class from that supposedly active in the actual conduct of the subject in making his judgments, as previously indicated. The attention operative was probably more nearly allied to a process of half-activating images in order to facilitate perception. This is a 'Balance' rather than an 'Initiating' function according to the terminology of this paper. It also seems that attention may involve an active inhibition of all bodily movements except those actually necessary for the particular programme of conduct in action. It may involve a similar inhibition of such movements as have a definitely disadvantageous effect—such as excessive postural tone which leads to roaring in the ears, and a truncation of all psychic processes that arise spontaneously, at once, if they are irrelevant, so that the subject would be occupied with thinking about a succession of problems each inhibited as soon as it arises. This might degenerate into a day-dreaming which did not interfere with the experiment, and that in turn be a step in the direction of dreaming and sleep. If the attempt to truncate all irrelevant thoughts does not lead to day-dreaming, it will probably lead to confusion and irritation. We thus return to the problem of affect. One would tend to regard attention as an 'Initiative' effort to imagine an expected situation and to prepare for the reaction it will entail. The interest of this experiment is that the whole conduct of the subject was so simplified that, unless he could employ an extremely primitive type of attention, he merely produced mal-adaptive derivations.

Conclusion.

It is evident that though the primary objects of this experiment were but little achieved, secondary questions of great interest developed, and lead to a discussion of the psychological conditions of reactions, and through them to a suggestion as to the mode of development, and relative status, of different types of reaction, and also to a consideration of the relative status of certain methods of scientific approach to organic problems.

There is reason to suppose that a further pursuit of this wide-embracing method might lead to very interesting results bearing upon the various theories of action that can be elaborated. The experimental method is unquestionably a help in formulating and deciding many problems that arise, and would still be useful in this respect, even if, in psychology, it never achieved the position granted to it in some other sciences.

A main result indicated appears to be that the psychologist is quite justified in refraining from merging his science into Biology or Physiology, as he is sometimes pressed to do.

NOTE. In so far as the experiment dealt with a possible relation between seeing colours and hearing sounds it was undertaken at the suggestion of Mr Vaudrey Mercer of Trinity College, Cambridge, and he collaborated with the writer throughout. I wish to thank Mr F. C. Bartlett and Dr H. Banister for valuable advice and criticism.

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THE TETRAD-DIFFERENCE CRITERION

BY GODFREY H. THOMSON.

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I. *Introduction* (pp. 235, 236).
II. *The tetrad-difference formulae* (pp. 236, 237).
III. *Application to Simpson's data* (pp. 238–241).
IV. *Application to dice-throws* (pp. 242–246).
V. *Another application to dice-throws* (pp. 246, 247).
VI. *The Gates' set of correlation coefficients* (pp. 247–250).
VII. *Peculiarities of the Gates' data* (pp. 250–252).
VIII. *General tendency among correlation coefficients for F to equal zero* (pp. 252–255).

I. *Introduction.*

THE main object of this paper is to apply the new tetrad-difference criterion to correlational data arising from variables of known structure, in order to ascertain its powers of diagnosing those structures. This criterion is one of the most recent steps in Prof. Spearman's investigations into the nature of ability.

More than twenty years ago he propounded the "Theory of Two Factors" as an explanation of the correlations found between various mental activities; a theory which declares that with only minor exceptions all such correlations are due to '*g*,' running through all our mental activities, and *not* to group factors running through some only. Each activity is on this theory due to '*g*' and to its own special specific factor '*s*' which (with the minor exceptions noted above) is only operative in that particular activity. Very vividly Prof. Spearman has suggested that '*g*' is intellectual energy and '*s*' the particular neural machine through which that energy is on any occasion being utilized.

It is necessary to note that it is the absence or unimportance of group factors which is the essential point of this theory. For from *any* set of correlation coefficients, provided they are positive (as is usual in mental tests), one can without fear of contradiction presume the presence of a general factor. Unless however the coefficients show a certain relationship to one another, group factors must also be present[1], and Prof. Spearman's work has been directed to elucidating this relationship and discovering means of determining it.

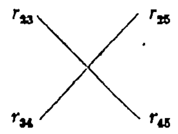
There have been three stages in this attempt. Before 1912 the existence of the relationship referred to was determined by inspection, which naturally led to disagreement. In 1912 Hart and Spearman[2] introduced the 'columnar correlation' criterion, which has now, since 1924, been discarded in favour of the 'tetrad-difference' criterion which is the subject of the present paper. These stages have undoubtedly been advances.

II. *The Tetrad-difference Formulae.*

When several mental tests (say five) are intercorrelated the coefficients can be arranged in a square table such as this:

	x_1	x_2	x_3	x_4	x_5
x_1	—	r_{12}	r_{13}	r_{14}	r_{15}
x_2	r_{12}	—	$\mathbf{r}_{23}$	r_{24}	$\mathbf{r}_{25}$
x_3	r_{13}	r_{23}	—	r_{34}	r_{35}
x_4	r_{14}	r_{24}	$\mathbf{r}_{34}$	—	$\mathbf{r}_{45}$
x_5	r_{15}	r_{25}	r_{35}	r_{45}	—

Then any four of these which form a rectangle, say those printed heavily in the above table, are called a *tetrad*:



of which tetrads there are in larger tables very many indeed; and the tetrad-difference is the quantity

$$r_{23}\,r_{45} - r_{34}\,r_{25} \equiv F \qquad \ldots(1).$$

It was shown many years ago by Prof. Spearman that these tetrad-differences all equal zero when there are no group factors, and in 1922 he published a more rigorous proof including a proof of the converse[3]. Since however in actual experiment departures from zero would naturally occur owing to sampling, it was necessary to know the probable error of this quantity, and this want was supplied by Spearman and Holzinger in 1924[1] when they published the following formulae:

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[1] See however section VIII.

[2] This *Journal*, 1912, v, 51.

[3] *Proc. Roy. Soc.* 1922, A, CI, 97–100. But see below, section VIII.

$$\text{P.E.}_F = 1{\cdot}349\, p/\sqrt{N} \qquad \ldots(2)$$

where $p = \bar{r}(1 - \bar{r})$, $N =$ number of subjects, and $\bar{r}$ is the mean of the four r's concerned in the tetrad. A better approximation was given as

$$\text{P.E.}_F = 1{\cdot}349\sqrt{\frac{p^2}{N} + \frac{(1-\bar{r}^2)^4}{2N^2}}, \qquad \ldots(3)$$

which gives a larger value. These formulae are intended to be applied separately to each tetrad difference[2]. This procedure, although in my opinion the only legitimate one, is obviously very laborious, and in a supplementary paper[3] Spearman and Holzinger give a formula which deals simultaneously with all the tetrad-differences derivable from the whole set of correlations, viz:

$$\text{P.E.}_F = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2} \qquad \ldots(4)$$

where $s^2 =$ the average squared deviation from $\bar{r}$, now the average of *all* the correlations.

Spearman and Holzinger state that they have almost always found this formula quite adequate. However they add two further formulae:

$$\text{P.E.}_F = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2 + \frac{12ps^2}{(n+1)(n-2)}} \qquad \ldots(5)$$

to be used when either $\bar{r}$ is under 0·2 or n, the number of variables, is under 6, and

$$\text{P.E.}_F = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2 + \frac{12ps^2}{(n+1)(n-2)} + \frac{(1-\bar{r}^2)^4}{2N}} \qquad \ldots(6)$$

if either* $\bar{r}$ is under 0·2 or N, the number of subjects, is under 50. Although it is a little disquieting to notice that these formulae progressively increase the probable error, and never decrease it, they form a weapon of investigation which is worthy of most careful examination and trial.

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[1] This *Journal*, 1924, xv, 17.

[2] They will be found so applied *e.g.* by H. G. Stead in this *Journal*, 1926, xvi, 209. On Prof. Spearman's suggestion he also applied to each tetrad-difference the formulae (4) and (5) which follow.

[3] This *Journal*, 1925, xvi, 86.

* This seems to leave in doubt whether (5) or (6) is recommended when $\bar{r}$ is under 0·2 and n and N larger than 6 and 50.

III. *Application to Simpson's data.*

One of the first published investigations using the tetrad-difference criterion was that employed as an illustration by Prof. Spearman in his presidential address to the Psychology Section of the British Association in 1925[1] where he examined by its means a table of correlations found by Simpson[2] and already previously tested by the columnar correlation criterion now discarded[3].

For Simpson's table, containing 14 variables and dependent upon 37 subjects, Prof. Spearman had the 3003 tetrad-differences calculated, while he himself calculated the probable error, that is, the *median* value that should by his theory be expected to arise from sampling alone, and found it to be 0·061. The *average* value of the 3003 tetrad-differences actually derived from the table (taken regardless of sign, that is) was 0·074, which when brought to the *median* value by multiplication by the well-known constant 0·845 came to 0·062, a strikingly close agreement with the theoretical value 0·061.

Prof. Spearman also gives a histogram[4] of the complete distribution of the tetrad-differences and remarks upon its excellent agreement with the normal curve based upon the theoretical P.E.

There can indeed be no doubt of the strongly 'hierarchical' nature of Simpson's table, and the slight modifications I am about to make in Prof. Spearman's presentation of the results will not I hope be taken as criticisms or as controversy, of which I am heartily sick, but as necessary in view of the comparison I am going to draw later between this table of Simpson's and some dice correlations of my own.

The remarkable symmetry of his histogram[5] is liable to mislead even a careful reader. It is possibly intended to represent the tetrad-differences calculated regardless of sign, thus giving one half of the histogram, which has then perhaps been duplicated for illustrative purposes.

With the assistance of my wife I have recalculated these tetrad-differences and as we worked over the whole square table we obtained each F twice and were thus able to check the accuracy of our work. We obtained the following distribution:

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[1] *Report* of the 93rd Meeting of the British Association for the Advancement of Science, p. 176 *et seq.*

[2] Simpson, *Columbia Contributions to Education*, Teacher's College, No. 53.

[3] Spearman, *Psychol. Rev.* 1914, XXI, 101.

[4] The scale in his diagram is wrongly given as 0·010, 0·020, 0·030 whereas it should be 0·10, 0·20, 0·30: and by another misprint on the same page 177 of the *Brit. Ass. Report* 0·0845 is given for 0·845.

[5] *Brit. Ass. Report*, 1925, p. 177.

TABLE I. (*See* Fig. 1.)[1]

F	minus	plus	*F*	minus	plus
0·3750–0·3999	0	1	0·1750–0·1999	31	21
0·3500–0·3749	3	2	0·1500–0·1749	33	62
0·3250–0·3499	3	4	0·1250–0·1499	79	82
0·3000–0·3249	5	12	0·1000–0·1149	97	102
0·2750–0·2999	13	18	0·0750–0·0999	150	233
0·2500–0·2749	10	25	0·0500–0·0749	206	307
0·2250–0·2499	28	26	0·0250–0·0499	298	361
0·2000–0·2249	25	31	0·0000–0·0249	328	407

This gives the following histogram (Fig. 1) which is there compared with the same normal curve which appears on Prof. Spearman's form of the diagram, *i.e.* one of P.E. = 0·061.

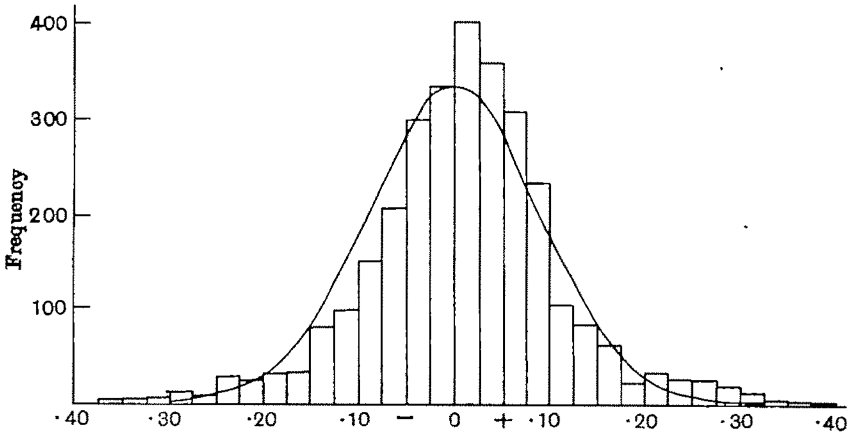

Fig. 1. Simpson's Data, Mental Tests.

For the mean value of these tetrad-differences regardless of sign we obtained 0·0739 agreeing with 0·074 found by Mr Raper for Prof. Spearman and thus indirectly confirming our calculations.

$$\begin{array}{lll} \text{The algebraic mean} & & = +\,0{\cdot}0098 \\ \text{About this mean,} & 0{\cdot}6745\sigma = & 0{\cdot}067 \\ \text{About zero} & 0{\cdot}6745\sigma_1 = & 0{\cdot}068 \end{array}$$

The median tetrad-difference by counting, regardless of sign, is in the group above 0·0500, and by interpolation in that group is found to be 0·0677.

---

[1] There were five tetrad-differences exactly zero, which are included in the 407 cases given as plus, in all calculations: but in the diagram which follows they have been transferred to the negative side.

For the purpose of finding the theoretical P.E. by the various formulae I calculated the following values:

$$91\bar{r} = 44{\cdot}29$$

$$91s^2 = 3{\cdot}9604$$

$$1{\cdot}349p/\sqrt{N} = 0{\cdot}0554 \qquad \text{from (2)}$$

$$\frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + \frac{(1-\bar{r}^2)^4}{2N}} = 0{\cdot}0574 \qquad \text{from (3)}$$

$$\frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2} = \frac{0{\cdot}43906}{\sqrt{37}} = 0{\cdot}072 \qquad \text{from (4)}$$

I have been unable to obtain, with Spearman, the value 0·061, but possibly this value is from some other approximate formula. The discrepancy is not great and Prof. Spearman himself doubtless lays no great stress on the dramatic agreement of 0·062 with 0·061. The above values from (2) and (3) are not really applicable to the whole set of correlations taken simultaneously and are only given here in illustration of the search for the value 0·61. The comparison of theory with actuality according to Spearman is

0·062 (actual), 0·061 (theoretical)

and we might in similar fashion compare

$$0{\cdot}6745\sigma_1 = 0{\cdot}068 \text{ (actual)}$$

and $$\frac{1{\cdot}349}{N}\sqrt{p^2 + s^2} = 0{\cdot}072 \text{ (theoretical)}.$$

Either way, there is the closest agreement between theory and actuality. But the distribution is not, if my calculations are correct, as symmetrical as Prof. Spearman's diagram leads one to think, nor does it fit so closely the theoretical curve.

If we take five times the theoretical P.E. we have either

$$5 \times 0{\cdot}061 = 0{\cdot}305 \text{ (Spearman)}$$

or $$5 \times 0{\cdot}072 = 0{\cdot}360 \text{ (our calculation)}$$

and very few of the actual tetrad-differences lie outside these limits, only one or two according to the second number, about 30 (out of 3003) by Spearman's value. With the exception of these cases, the others may, according to the theory, be accepted as only sampling departures from zero. An investigation of the few cases of high tetrad-differences leads to the identification of certain correlations as being chiefly involved in their production, and of certain of the variates as having therefore small group factors. But in the main '*g*' and the 'specifics' are a sufficient explanation.

The reader may perhaps however recall that I have previously opposed similar conclusions of Prof. Spearman's on the general ground that, although a perfect compliance with the relationship now measured by $F = 0$ was a practical proof of the absence of group factors, yet a random complexity of numerous group factors gave such a good approach to compliance that never yet had an experiment been performed on a sufficient number of subjects, and with sufficient reliability, to distinguish the two cases. That is still entirely my opinion, and my belief is that the mind is such a complexity, and not a general energy plus an enormous number of discrete and separate specific abilities. I recognise the existence of general ability in the sense that most mental activities are positively correlated, but I do not admit as yet any proof that these positive correlations are all due to one universally applicable '*g*' and (with minor exceptions) to nothing else. Even assuming the entire accuracy of Prof. Spearman's new formulae—and I feel grave doubts about the form which employs all the correlations simultaneously—we may not merely take cases where, because the subjects are few, the P.E. is large. It must be shown that tables of correlation coefficients, which depart from strict compliance with $F = 0$ only (it is said) because the sample is small, do actually approach steadily towards more strict compliance when the sample is increased. The proper plan would be to calculate theoretically, and observe practically, the sigma of F as $N = 100$, 200, 300, up to 1000 cases and see whether convergence or divergence was indicated.

I do not say this in a controversial spirit, but with a real hope of advancing our science by maintaining a critical attitude. My scepticism is upheld by the fact that when I apply these mathematical methods—and I believe thoroughly in mathematical methods and am sure we can never advance without them—when I apply them not to psychological data where we are ignorant but to other data where we know the facts, I find them incapable, with samples such as are commonly used, of diagnosing those facts. The methods may be all right, but we must have data with smaller probable errors before we can draw definite conclusions.

The other data to which I have just referred are structures of dice throws to which I have previously applied the 'columnar correlation' criterion and to which I propose now to apply the new tetrad-difference formula.

IV. *Application to dice-throws.*

In various papers I have described methods of making *random* arrangements of dice throws, where a score (imitating a mental test score) is made up of the number of pips on a throw of several dice, and wherein certain dice may be counted, as they lie, into several scores thus imitating group factors. These will be found summed up in Chapter X of *The Essentials of Mental Measurement*[1] where references will be found to the original papers.

I would like to emphasize the random nature of the formation of these dice throws, as one form of reply to my present argument has been to speak as though I had at great pains made a set of factors to imitate in some measure hierarchical order giving zero tetrads. Such a misconception is no doubt due to my having on the first occasion done this: but I soon found that mere random complexity was all that was necessary. Everything was left to depend on the cutting of a pack of cards or the throw of dice, and any reader can repeat the experiment as described. It has indeed been done in classes in three universities, always with a similar result: and see also the theoretical proof in section VIII below.

The ten dice-variables described at length in the work cited were interconnected as follows:

	Group Factors (named after playing cards)													No. of specific dice
Variable	5	7	2	Q	K	8	Ace	4	10	Kn.	6	9	3	
x_7	×	×	×	×	×	×	×	×	×	×	×	×	×	13
x_3	×	×	×	×	×	×.	×	×	×	×	—	×	×	12
x_{10}	×	×	×	×	×	×	—	×	×	×	×	—	×	5
x_6	×	×	—	×	—	×	—	×	×	×	×	×	—	5
x_9	×	×	×	—	×	×	×	×	—	—	×	—	×	3
x_5	×	×	×	×	×	×	×	—	—	—	—	—	—	6
x_1	×	×	×	—	×	—	×	—	—	—	—	—	—	5
x_2	×	×	—	×	—	—	—	—	×	—	—	×	—	3
x_4	—	—	—	×	—	—	—	—	—	—	—	—	—	3
x_8	—	—	×	—	—	—	—	—	—	—	—	—	—	2

This figure has been rearranged from that given on p. 178 of Brown and Thomson, with which it is otherwise identical, in order to show as clearly as possible any approach to the Two Factor arrangement which may by a coincidence have occurred. It will be seen that the group factors 5 and 7 happen always to occur together and to run through eight of the ten variables. But this approximation to a small '*g*' is accompanied by a large number of group factors which run through only seven, six, five, or four of the ten, and are complexly interwoven. This is not the same thing, by any interpretation of terms, as a '*g*' and the absence of group factors, which is what the Theory of Two Factors by its very name implies.

---

[1] Brown and Thomson, Cambridge 1921 and 1925. The later edition corrects some arithmetical errors which did not however modify the argument.

The theoretical correlations arising from this complex structure are given in Brown and Thomson (p. 177) and observed values of these, produced by actually throwing the dice, on p. 178 (1925 edition) for $N = 30$ 'Subjects.' For these last coefficients I have calculated the tetrad-differences, aided by an Edinburgh post graduate class to whom I am greatly indebted; and again we worked over the entire table obtaining each F twice as a check. They are as follows (Table II and Fig. 2).

TABLE II. (*See* Fig. 2.)

F	minus	plus	F	minus	plus
0·2500–0·2778	1	2	0·1200–0·1299	14	6
0·2400–0·2499	3	0	0·1100–0·1199	10	9
0·2300–0·2399	1	3	0·1000–0·1099	15	4
0·2200–0·2299	2	6	0·0900–0·0999	24	4
0·2100–0·2199	4	2	0·0800–0·0899	18	6
0·2000–0·2099	7	1	0·0700–0·0799	32	10
0·1900–0·1999	5	3	0·0600–0·0699	35	11
0·1800–0·1899	6	5	0·0500–0·0599	32	26
0·1700–0·1799	2	8	0·0400–0·0499	36	24
0·1600–0·1699	4	3	0·0300–0·0399	42	17
0·1500–0·1599	5	7	0·0200–0·0299	20	22
0·1400–0·1499	11	8	0·0100–0·0199	19	22
0·1300–0·1399	7	5	0·0000–0·0099	27	34

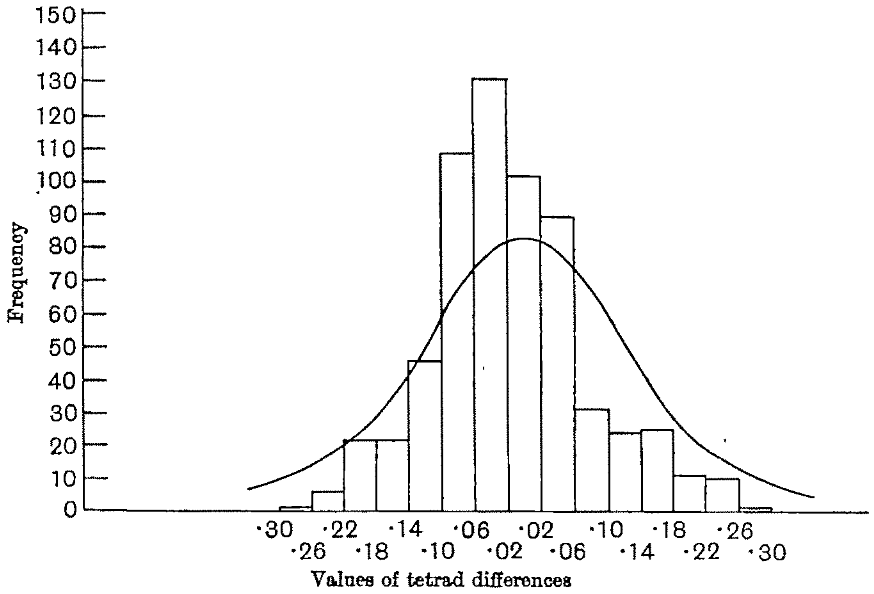

Fig. 2. First Dice Data, 30 subjects.

The mean variation of these from zero, regardless of sign, is 0·07522, which when multiplied by 0·845 to bring to the median or P.E. gives 0·06356. This is the observed value, and according to the argument advanced by Prof. Spearman, if this is not greater than the theoretical value, the absence of group factors is proved.

For the calculation of the theoretical value we have:

$$45\bar{r} = 14{\cdot}05$$
$$45s^2 = 2{\cdot}96218$$
$$N = 30$$

whence

$$\text{P.E.}_F = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2} = 0{\cdot}082.$$

The actual distribution is thus well within the limits of sampling error and is therefore as much a proof of the absence of group factors as is Simpson's table. But in this case we know that the facts are otherwise.

On p. 177 of Brown and Thomson is given a table of the *theoretical* correlation coefficients corresponding to the structure diagram given in the present section. The following Table III and Fig. 3 show the distribution of the *true* tetrad-differences of which Table II and Fig. 2 give the *observed* counterparts.

TABLE III. (*See* Fig. 3.)

F	minus	plus
0·1800 to 0·1822	1	0
0·1400 to 0·1799	2	5
0·1000 to 0·1399	4	15
0·0600 to 0·0999	45	59
0·0200 to 0 0599	135	149
−0·0199 to +0·0199	215	

Fig. 2 has really splashed out from Fig. 3, and not from zero as the Spearman formulae would have us believe.

The real conclusion of course is that with 30 or 37 subjects the data as observed have such large probable errors that they are consistent with either theory, and prove neither. And it is perfectly clear that a very large increase indeed in the number of cases would be required to enable a discriminating conclusion to be drawn.

Before passing on to a case with twice as many 'subjects' an attempt may be made, on orthodox lines, to discover whether any small group factors are indicated by the observed tetrad-differences. Strictly speaking, none are, for the distribution is well within 5 × ± 0·082. But

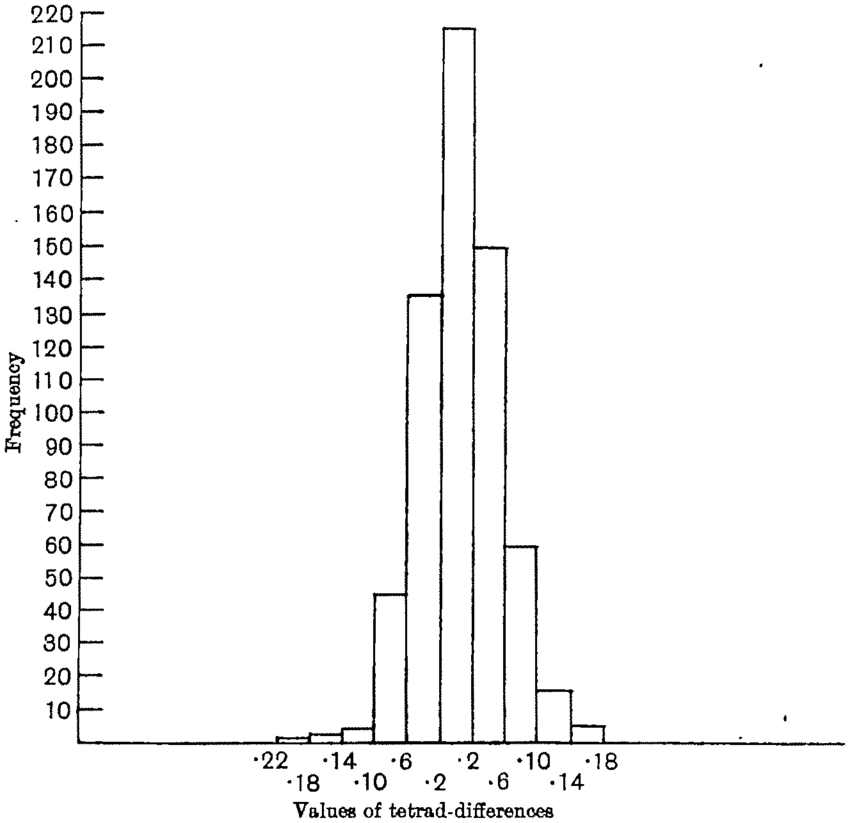

Fig. 3. First Dice Data, Theoretical Values.

let us examine the largest tetrad-differences. Those over ± 0·2400 (about 3 × P.E.) are as follows:

– 0·2778	produced by	$(x_1 x_2)(x_6 x_3)$
+ 0·2733	,,	$(x_{10} x_1)(x_6 x_3)$
+ 0·2580	,,	$(x_1 x_2)(x_6 x_9)$
– 0·2456	,,	$(x_3 x_2)(x_6 x_1)$
– 0·2451	,,	$(x_{10} x_4)(x_5 x_7)$
– 0·2410	,,	$(x_{10} x_1)(x_6 x_9)$

Every variable except x_8 appears somewhere in this list: but the correlations most often concerned in the production of these large tetrad-differences are

r_{16}	Four times.
r_{13}	Three times.
r_{26}	Three times.
$r_{10,6}$	Twice.
r_{19}	Twice.

This certainly throws suspicion on to the variables x_8 and x_1 especially, and a great advantage of this tetrad-difference method, and one on which Prof. Spearman is to be congratulated, is the possibility which it thus unfolds of 'spotting' special factors. But an examination of the actual structure diagram shows no special connection whatever of x_8 and x_1 other than connections which they share with many other variables, and it is pretty clear that these particular tetrad-differences are large merely by accident.

V. *Another application to dice-throws.*

In *Biometrika*, 1923, xv, 155–60 is described another instance of a similar random complexity of group factors which had previously been mentioned without description in 1919. Without reprinting the tables there given it will be sufficient here to give the tetrad-differences and make the comparison with the theoretical distribution according to the Theory of Two Factors. The number of 'subjects' here was $N = 60$.

TABLE IV. (*See* Fig. 4.)

F	minus	plus
0·2000–0·2199	2	2
0·1600–0·1999	15	4
0·1200–0·1599	20	13
0·0800–0·1199	44	20
0·0400–0·0799	91	87
0·0000–0·0399	156	176

The mean deviation from zero, regardless of sign, is 0·0499[1], and thence

$$\text{observed P.E.} = 0{\cdot}0499 \times 0{\cdot}845 = 0{\cdot}0422.$$

For the theoretical P.E. we have

$$45\bar{r} = 16{\cdot}22$$

$$45s^2 = 1{\cdot}03539$$

$$N = 60$$

$$\frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2} = 0{\cdot}048.$$

Here again therefore with 60 subjects the observed P.E. is no greater than that given by Prof. Spearman's formula, although the cause of correlation, instead of being '*g*,' is a complexity of group factors. The histogram, and the theoretical normal curve of P.E. 0·048, are shown in Fig. 4. No tetrad-difference is greater than 5 P.E.

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[1] It was calculated from a finer grouping than is here shown.

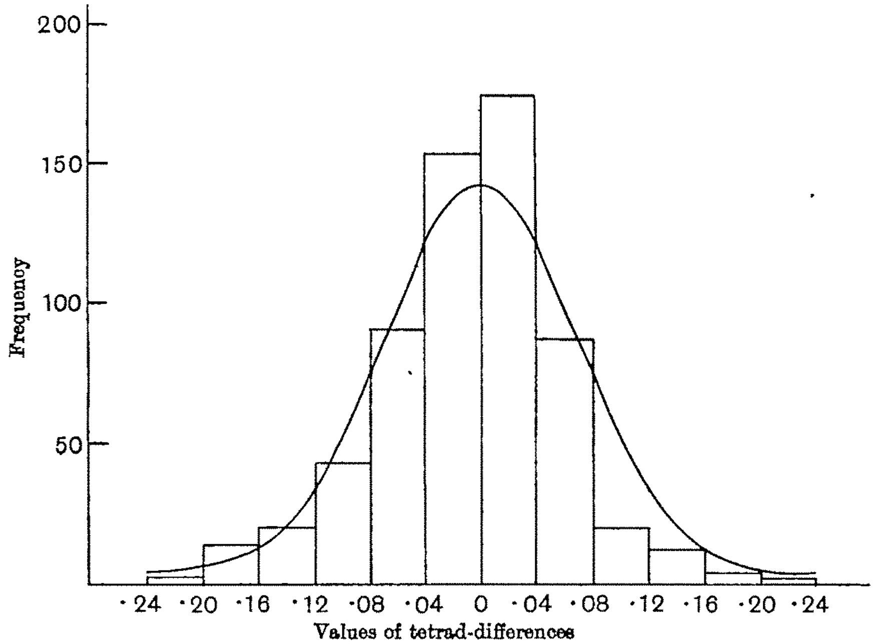

Fig. 4. Another set of Dice Data, 60 subjects.

It seems therefore that it is impossible with 60 cases to distinguish Two Factors from a complexity of group factors, and further that a very much larger number of subjects than 60 is necessary.

VI. *The Gates' set of correlation coefficients.*

In his British Association address however Prof. Spearman made another important contribution to the enquiry, by taking a table of correlation coefficients from another province than mental measurement and showing that this table was very far from satisfying his tetrad-difference criterion. The table was taken from a paper by Prof. Arthur I. Gates[1] and refers to physical not mental traits:

	1	2	3	4	5	6	7	8
1. Area of ossification	—	88	60	62	43	31	25	26
2. Ratio of ossification	88	—	52	58	41	21	24	29
3. Height	60	52	—	69	44	51	45	11
4. Weight	62	58	69	—	65	39	40	83
5. Chest girth	43	41	44	65	—	59	36	69
6. Lung capacity	31	21	51	39	59	—	46	26
7. Strength of grip	25	24	45	40	36	46	—	14
8. Nutrition	26	29	11	83	69	26	14	—

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[1] *Journ. Educ. Psychol.* September 1924, p. 339

For this table Prof. Spearman finds, for the actual median tetrad-difference 0·089 and for the theoretical value from his theory 0·011, the actually observed value being eight times the theoretical, instead of equal to it as in the case of mental measurements. He makes the comparison graphically in a figure on p. 178 of the *Brit. Ass. Report*, 1925, where there is this time no agreement whatever between the theoretical curve and the actual histogram.

As however I am again unable to obtain Prof. Spearman's results exactly I must, before discussing this case, give the results which I obtain from my calculations on the Gates' correlations, and a figure, only saying, before doing so, that this Gates' table has not to my knowledge been brought forward, as Prof. Spearman's words might suggest, as an example which satisfies the mathematical criterion. Prof. Gates merely says in his paper that the table "looks not unlike a chart of inter-correlations among specific mental tests" and does not mention Prof. Spearman.

Again we found each tetrad-difference twice, as a check, by working over the whole square table. The distribution is as follows:

TABLE V. (*See* Fig. 5.)

F	Minus	Plus
0·5500–0·5796	—	2
0·5000–0·5499	—	1
0·4500–0·4999	—	1
0·4000–0·4499	—	3
0·3500–0·3999	—	5
0·3000–0·3499	2	7
0·2500–0·2999	2	10
0·2000–0·2499	7	12
0·1500–0·1999	2	15
0·1000–0·1499	12	20
0·0500–0·0999	18	32
0·0000–0·0499	28	31

Drawn to a suitable unit this gives Fig. 5, which shows the ordinary kind of scatter histogram whereas Prof. Spearman's diagram, by putting all the cases beyond 0·055 into rectangles on narrow bases, gives the impression of an abnormally centrifugal distribution.

When these tetrad differences are taken regardless of sign their mean deviation from zero is 0·1333 and this multiplied by 0·845 gives

$$0{\cdot}1127$$

where Spearman obtains 0·089. The tetrad-differences depart according to my calculation even more from zero than he thought.

Turning now to the theoretical calculation I obtain

$$28\bar{r} = 12{\cdot}57$$

$$p^2 = 0{\cdot}0612028$$

$$s^2 = 0{\cdot}0381453$$

$$\frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2+s^2} = 0{\cdot}040 \text{ for } N = 115.$$

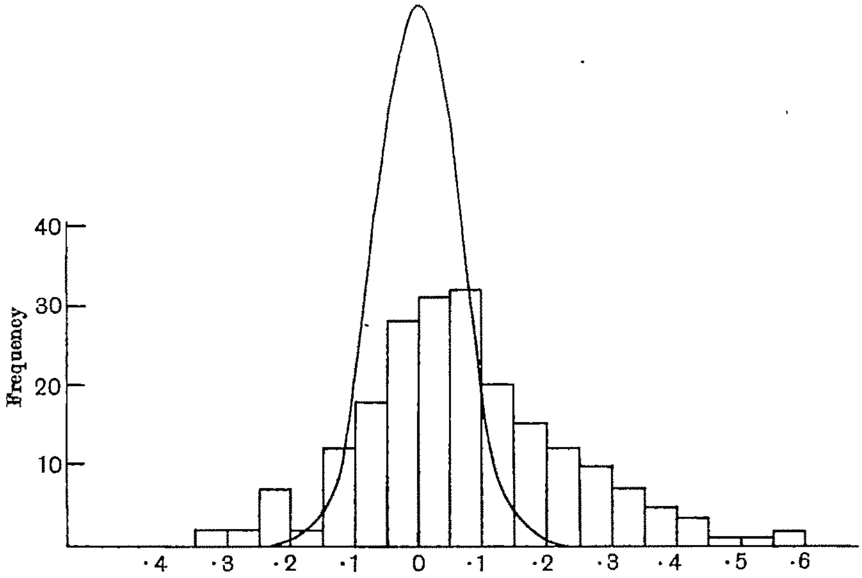

Fig. 5. Gates' Data, Physical Traits.

Here the discrepancy is serious, for the value given by Prof. Spearman is 0·011. The comparison of theory with observation is:

Theory	Observation
0·011	0·089 Spearman.
0·040	0·113 Thomson.

According to my arithmetic, while there is still a considerable difference between theory and observation, it is nothing like so large as Prof. Spearman thought. The observed value is not eight times, but less than three times the theoretical. To enable my arithmetic to be more readily checked I give some intermediate results

$$\bar{r} = 0{\cdot}44893$$

$$\Sigma r^2 = 6{\cdot}7111 \text{ (28 coefficients)}$$

$$\Sigma^2 r = 158{\cdot}0049.$$

The normal curve centred at zero and with P.E. = 0·040 is shown on Fig. 5 for comparison with the histogram. A comparison with Fig. 1 (Simpson's data) shows clearly enough that there is *there* much greater agreement between theory and observation than *here*, even after the arithmetical changes have been made which I have indicated.

But before discussing further the causes of this difference, I think I may even now fairly claim that these other physical data do show *some* tendency towards hierarchical order, that is do show a tendency towards zero tetrad-differences. Why this tendency is in this particular case only weak I hope presently to suggest: but it is at least not entirely absent as Prof. Spearman's diagram led one to imagine.

VII. *Peculiarities of the Gates' data.*

There are however many points about these data which make it very unlikely that good agreement should be found, points of a nature which would certainly be objected to by Prof. Spearman were these mental data.

For example, the first variable, 'area of ossification,' actually forms the arithmetical numerator of the second variable, 'ratio of ossification[1].'

'Chest girth' and 'lung capacity' are surely likely to have a very obvious special connection.

'Nutrition' is the difference between certain norms and the variable 'weight[2],' so that there is again an obvious connection. If we examine the seven largest tetrad-differences we find that six of them are concerned with 'nutrition,'' and the seventh with 'ratio of ossification.' If we separate the tetrad-differences which do, from those which do not, contain the variable 'nutrition' we find that the 105 tetrad-differences of the former sort range from − 0·3272 to 0·5796, whereas the 105 of the latter sort range only from −0·1599 to 0·4289. Clearly this variable 'nutrition' is unduly increasing the scatter, because probably of its close connection, even arithmetically, with 'weight.' And possibly the other two connections noted are less extreme cases of the same thing, especially 'ossification' and 'ratio.' When there are such connections, it has been Prof. Spearman's practice to pool the two tests or omit one of them: but that proceeding here would leave us with only five variables and only fifteen tetrad-differences, or 45 if we leave 'chest girth' and 'lung capacity' separate.

However, if one does omit the variables 'ratio of ossification' and 'nutrition,' there is not, it must be confessed, a very great improvement in the agreement of observation and theory. The observed P.E. of the 45 tetrad-differences is then 0·078, the theoretical is 0·033.

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[1] Gates, *cp. cit.* p. 333.

[2] Gates, *op. cit.* p. 333.

One other doubtful point remains. The number of subjects is given as $N = 115$. Now it is true that 115 children were measured. But the correlations were not calculated from 115 cases direct. The boys and girls, the kindergarten and Grade IV, were treated separately, in four groups of subjects numbering 30, 28, 27 and 30 respectively. Each r in Gates' table is the average of the four r's thus obtained. I have some doubts whether it is then right to use $N = 115$ in the Spearman criterion, and it is striking that $N = 30$ gives, with my calculations, almost satisfactory agreement of theory and observation.

It is true that the P.E. of r is given by $0{\cdot}6745\,(1 - r^2)/\sqrt{N}$, and that the P.E. of the average of four r's would be $\sqrt{4}$ smaller. But that is when these four r's are four measurements of the same thing. Here the four groups differ in sex, and in age by about four years (two groups were about 5½ years, two about 9½ years), and this may have an influence.

The four r's, each based on from 27 to 30 subjects, whose averages form Gates' table[1].

	2	3	4	5	6	7	8
1. Area of ossification	89	59	69	49	29	28	38
	87	67	53	44	22	20	12
	86	65	67	43	35	39	30
	90	47	57	38	40	11	23
2. Ratio of ossification	—	57	63	40	23	25	35
	—	65	56	32	12	17	20
	—	49	49	41	29	32	27
	—	37	65	49	19	21	36
3. Height	—	—	81	45	40	50	10
	—	—	70	38	52	40	01
	—	—	67	43	62	40	21
	—	—	58	50	50	52	13
4. Weight	—	—	—	66	45	50	68
	—	—	—	51	34	35	85
	—	—	—	80	31	36	86
	—	—	—	64	48	41	93
5. Chest girth	—	—	—	—	54	50	61
	—	—	—	—	66	40	48
	—	—	—	—	52	30	79
	—	—	—	—	63	25	88
6. Lung capacity	—	—	—	—	—	61	24
	—	—	—	—	—	42	33
	—	—	—	—	—	36	18
	—	—	—	—	—	44	28
7. Strength of grip	—	—	—	—	—	—	15
	—	—	—	—	—	—	07
	—	—	—	—	—	—	26
	—	—	—	—	—	—	09

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[1] Each r was a partial r, age having been eliminated by the usual formula.

Prof. Gates kindly sent me a copy of the separate correlations (omitted in his paper, though referred to as 'Appendix'—the Appendix having later been deleted). An inspection of these is in the main reassuring to Prof. Spearman on this point, for the r's agree as well as can be expected. They are given at the foot of the preceding page.

VIII. *General tendency among correlation coefficients for F to equal zero.*

To sum up, we have seen that Simpson's mental data agree, if not quite perfectly as Prof. Spearman thought, yet almost so, with the theory that the tetrads differ from zero only by sampling errors. But we have also seen that dice data formed from a complexity of group factors agree quite as perfectly. Indeed the scatter of tetrad-differences from this source is sensibly *less* than Spearman's formula

$$1{\cdot}349\sqrt{(p^2+s^2)}/\sqrt{N}$$

and this arouses a certain doubt in my mind as to the adequacy of the latter.

So far this agrees with the theory I have always put forward, that approximation to hierarchical order, or a tendency to zero tetrad-differences, is as natural a phenomenon among correlation coefficients as is normal distribution among measurements of one variable. Normal distribution, or an approach to it, is found wherever the underlying causes of variation are numerous and random; and under similar conditions arises also an approach to zero tetrad-differences. Any *departure* from either phenomenon requires explanation, not their occurrence.

A common method of regarding normal distribution, and the method used by Gauss, is to consider the variable quantity as dependent upon a large number of causes, each small, and each as likely to be present as absent. In one kind of measurement these tiny elements are 'errors': but in another kind they are true causes, and the variations are real variations. This assumption leads directly to the normal curve of probability.

If in similar fashion we suppose that four correlated variables depend upon an underlying complex of elemental factors, each variable being a group of these factors, we find that the most probable value for the tetrad-difference F is zero, without any restrictions whatever on the linkages, and especially without any restriction whatever to the case where all the correlations are due solely to a 'g.'

Let the underlying possible factors be A in number, and let the variable x_1 depend upon any p_1A of these, x_2 upon p_2A, x_3 upon p_3A, and x_4 upon p_4A, where the p's are proper fractions.

Then the most probable number of factors shared by x_1 and x_2 will be $p_1\,p_2A$. The value[1] of the correlation of x_1 and x_2 will be

$$r_{12} = \frac{\text{number of common factors}}{\sqrt{(\text{total factors in } x_1 \times \text{total factors in } x_2)}}.$$

(In this the assumption is made that the factors have equal values, otherwise certain sigmas and weights would complicate the formula.)

That is to say, the most probable value is

$$r_{12} = \frac{p_1\,p_2A}{\sqrt{p_1A \times p_2A}} = \sqrt{p_1\,p_2},$$

and similarly

$$\begin{aligned} r_{14} &= \sqrt{p_1\,p_4}, \\ r_{23} &= \sqrt{p_2\,p_3}, \\ r_{34} &= \sqrt{p_3\,p_4}. \end{aligned}$$

In the tetrad

	x_2	x_4
x_1	r_{12}	r_{14}
x_3	r_{23}	r_{31}

we have therefore

$$\begin{aligned} F &= r_{12}\,r_{34} - r_{23}\,r_{14} \\ &= \sqrt{p_1\,p_2}\sqrt{p_3\,p_4} - \sqrt{p_2\,p_3}\sqrt{p_1\,p_4} \\ &= 0, \end{aligned}$$

and similarly with the other tetrads possible. *Zero is in every case the most probable value.*

The difference between the Theory of Two Factors, and the theory that the variables are samples of a complex of factors, is not, therefore, that the former gives $F = 0$ while the latter gives a different value for F. Both give $F = 0$. The difference is that on the former theory *all* tetrad-differences are *exactly* zero, while on the latter theory zero is only the most probable value, the actual *theoretical* values being distributed about zero. In both cases the *observed* values will be distributed about zero or some adjacent point; but the scatter ought to be less on the Theory of Two Factors, which starts with all the F's true zeros, than on the theory that each variable is a sample of a complex of factors, where the theoretical F's, though for each the most probable value is zero, are already distributed about it. The dice examples given show however that by Prof. Spearman's formulae no differentiation can be made at any rate up to 60 subjects.

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[1] Thomson, *Brit. Journ. Psychol.* 1916, VIII, p. 275. Spearman, *ibid.* p. 282. See also Spearman, *Amer. Journ. Psychol.* 1904, XV, p. 75.

The physical data from Gates do nevertheless show a wider scatter from $F = 0$. It is true that Spearman by some error, as I think, much exaggerated the amount of departure, and that even here there is a tendency to zero tetrads. But the tendency is noticeably less than in the case either of the mental data or the dice-throws. I have suggested that this departure may be due to certain special linkages which disturb the random nature of the inter-connections, or that it may be bound up with the fact that the 115 children were measured in groups of about 30.

But if physical traits, and correlations from other sources of measurement, do actually prove on further inquiry to agree with $F = 0$ much less closely than do mental test data, this would not, on the way of looking at the matter which I uphold, be necessarily a proof of the existence of a '*g*,' and the absence of group factors, in mental traits, but only of the greater number of links, the greater complexity, found in mental activities. If a few pennies are thrown, the approximation to a normal curve produced by the histogram of their heads is less than if many are thrown. Similarly if a number of traits depend on very few underlying factors, their intercorrelations are less likely to show hierarchical order, or zero tetrads, than those of traits depending on a rich complexity of factors[1]. The mind, and the brain, supply such a rich complexity in mental tests, and I believe that the correlations found are compatible with such a thesis quite as well as with the Theory of Two Factors.

There remains a point which always seems to me to be a strong support to Prof. Spearman's Theory, and which I always expect him to emphasise more than he has done. That is the fact that transfer of training is so slight: for this is easily explicable on the Theory of Two Factors, by the thesis that '*g*' is unimprovable by training, that only the 'machine' can be trained, and that each 'machine' is specific.

I have on a former occasion attempted to offer an explanation of non-transfer on the 'complexity' theory[2]. To that I would now add the following consideration. If mental traits are indeed dependent on such a rich complexity of factors, then it may be that transfer experiments are more difficult to arrange than we realise. Almost everything depends on having a control group which differs from the trained group in nothing but the one antecedent in which we are interested. If however mental activities are dependent upon such interconnected linkages as are depicted in the arrangement shown on p. 242, or even more complicated linkages, then it becomes practically impossible to obtain a real control group, for although not trained in the particular activity, the same underlying factors are being trained by the other work which of necessity fills the time of the control group. Both groups will then improve in the test material, as in fact is invariably found, and the difference in favour of the trained group, which is usually slight, is on this view not a measure of transfer but the difference between two measures of transfer.

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[1] This will be illustrated in a later paper.

[2] *Psychol. Rev.* 1920, XXVII, p. 183: and Brown and Thomson, p. 190.

However, it is undesirable to close on a side issue, and the chief points of this paper have been (1) the fact that certain dice data, known to depend on random group factors which cannot be resolved into 'g' and 'specifics,' do nevertheless easily pass the Spearman and Holzinger new tetrad-difference criterion for the absence of group factors, even with 60 subjects; and (2) the presentation of a proof that for *any* four unknown variables depending on numerous equal, all-or-none, factors, the most probable value of a tetrad-difference between their coefficients of correlation is zero, and not only when the Theory of Two Factors is assumed[1].

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[1] *Final footnote added after reading the proof.* On reading this paper again after the lapse of some weeks I realize that to avoid appearing to claim more than is legitimate I ought to emphasize the fact that Section VIII refers throughout to underlying factors which are equal to each other in sigma and which are all-or-none in the sense that they act with their full strength, or not at all, in each variable, though that full strength may be different in different individuals, and in different factors in the one individual. I suspect that these restrictions are unnecessary, provided the factors are numerous and small, but they are used here. My reasons for thinking them unnecessary arise partly from certain trials of special cases, partly from the apparent generality of the later portion of my paper in *Proc. Roy. Soc.* 1919, XCV A when combined with p. 182 of the *Psychol. Review*, 1920, XXVII (reprinted in pp. 183–188 Brown and Thomson).

(*Manuscript received* 20 *September*, 1926.)

OBITUARY

Prof. BERNARD MUSCIO.

The news of the death of Professor Bernard Muscio early last year, at Sydney, came as a great shock to his friends in this country. Professor Muscio came from Australia to Cambridge several years ago, and took a degree there by research. Afterwards he carried out a number of experimental investigations in the Cambridge Psychological Laboratory the results of which were, in part, published in this *Journal*. He became for a time Demonstrator in Experimental Psychology in the University of Cambridge, returning to Australia later to take up a temporary post there. He then published his very well-known book on *Industrial Psychology*, and at the end of the war he returned to this country as an Investigator for the Industrial Fatigue Research Board. He was able to make Cambridge his headquarters and both he and Mrs Muscio took a lively interest in all the psychological and philosophical activities that were finding expression there at that time. He carried out and directed a number of researches of first-rate importance for the Industrial Fatigue Research Board, and his contribution both to this *Journal* and to the publications of the Fatigue Board became widely known and appreciated for their originality, their caution, their evidence of exact critical power, and their extreme accuracy alike of conception and of expression. In the end his philosophical interests claimed him once more and he returned to Australia to take up the Chair of Philosophy at the University of Sydney. Those who knew him best were most sorry to miss the stimulus of his criticism and understanding friendship, but they knew that wherever he went he would be outstanding as a man of rare mind and character. His death came early, with much of his work as yet unfinished.

All that he did he did well. He united to a most unusual degree a profound interest in and capacity for abstract thinking with the genuine experimentalist's loyalty to facts. This much anybody who reads his published work can tell; but above and beyond all that he was a man whose friendship nobody who ever had it will forget.

F. C. B.

PRELIMINARY REPORT ON A DEMONSTRATION OF EXPERIMENTS ON HYPNOTISM SHOWN BY GUSTAF WALLENIUS, FIL. MAG. (UPSALA)

At the 8th International Psychological Congress at Groningen (Sept. 6th–11th, 1926), Mr Wallenius gave a demonstration of experiments, which were a repetition of those performed by the late Prof. Alrutz. These experiments were claimed to show that hypnotism is the effect of a 'nervous effluence,' and that it is not, as ordinarily supposed, a psychological function.

At the first sitting (Sept. 9th) were present, as well as Mr Wallenius and the subject of the experiment (a Swedish metal-worker): Prof. Janet, Prof. McDougall, Prof. Roels, Dr van Loon, Prof. Spearman, Dr Thouless, and (for the last part of the demonstration) Mr Bartlett. The experiment which was claimed by Prof. Alrutz to be a crucial proof of his theory was performed under strictly controlled conditions. The next day, the same experiment was performed twice more in the presence of Mr Bartlett, Prof. K. H. Bouman, Dr van Loon, Dr Myers, Dr Thouless, and Prof. Zwaardemaker.

The methods and the results obtained will be fully reported in a longer communication which we hope to publish in the *Proceedings of the Society for Psychical Research* (London). We can, however, say definitely that no phenomena occurred in this demonstration which could require the theory of nervous effluence.

All those before whom the experiments were demonstrated agreed that the results obtained pointed to the ordinarily accepted theory of suggestion in hypnosis as an adequate explanation of all the phenomena observed.

F. G. H. VAN LOON.
R. H. THOULESS.

PUBLICATIONS RECENTLY RECEIVED

Emotion and Insanity. By S. THALBITZER. With a preface by Prof. HÖFFDING. London. Kegan Paul, Trench, Trubner & Co., Ltd. 1926. Pp. 128. 7*s.* 6*d.* net.

Dr Thalbitzer, who is chief of the medical staff at the Copenhagen asylum, finds an approach to the satisfactory classification and understanding of the emotions of normal people in the varieties of emotional condition in manic-depressive insanity. In Kraepelin's later classification of manic-depressive insanity with its complex types, there appear all the possible combinations of activity or passivity, productivity or unproductivity, and mania or melancholia. There are corresponding normal emotions which are the possible combinations of increased or decreased motor activity, increased or decreased mental activity, and pleasure or unpleasure. We thus get a schema of the emotions similar in structure to, but differing in detail from, the well-known tridimensional one of Wundt.

In addition to these affective processes of the first order, there are those of the second order in which pleasure and displeasure occur simultaneously. "For example, hope is as a rule principally a feeling of pleasure connected with the thought of some good which is desired and expected, but in it there is a more or less obvious displeasure-tone called forth by the thought that the hope may be disappointed" (p. 66). This conception of feeling processes in which pleasure and displeasure occur simultaneously and in which, apparently, the 'thought' of the object calling out these feeling tones is an integral part of the feeling process itself, seems to be open to grave objections which Dr Thalbitzer makes no attempt to meet.

The last chapter is mainly a polemic against Lehmann's theory of feeling. In this, Dr Thalbitzer appears to have the best of the argument. For those who have no tendency to accept Lehmann's theory, however, it is merely irritating to be compelled to read a refutation of it in a chapter which would have been more usefully occupied with a defence of Thalbitzer's own views.

R. H. T.

Human Nature and Education. By A. S. WOODBURNE. Oxford University Press. 1926. Pp. 292. 12*s.* 6*d.* net.

This admirable text-book on psychology comes from the Christian College at Madras. The greater part of it is an elementary treatment of general psychology with special emphasis on those aspects of psychology which are of pedagogical importance. Prof. Woodburne's exposition is clear, interesting and up-to-date, and should prove useful as an introduction to psychology for teachers and for candidates for degrees in education. Two chapters towards the end of the book deal more specially with educational problems: one on the psychological aspects of different educational systems, the other on methods of examination.

R. H. T.

The Language and Thought of the Child. By J. PIAGET. With preface by Prof. CLAPARÈDE. Translated by MARJORIE WARDEN. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1926. Pp. xxiii + 246. 10*s.* 6*d.* net.

This study of the speech and thinking of children between the ages of 4 and 11 was made by Prof. Piaget at the Institut Rousseau at Geneva. By the method of taking down all that was said during an extended period of time by different children and subjecting it to quantitative analysis, Piaget shows what a great part of the young child's speech is ego-centric and is not directed towards producing real results in his social environment. This ego-centrism rapidly decreases after about 7 or 8, when habits of social thought begin to be formed. The 'syncretism' of thought which results from this ego-centrism persists as a verbal syncretism even after the age of 7 or 8, and is the clue to what satisfies the older child as explanation.

It will be seen that the author is concerned with a problem related to what M. Lévy-Bruhl has called 'pre-logical thinking.' Prof. Piaget has, however, emancipated himself from the superstition of intellectualism which regards logic as the method of any thinking—whether civilized or uncivilized, of adult or child. Logic is a system of rules for the discipline of verbal expression in order to make it yield valid results. The increasing recognition of this fact makes possible the construction of a truly psychological natural history of thinking which is the necessary foundation for the adequate treatment of the still unsolved psychological problems of the origin and mode of production of beliefs. Prof. Piaget's work is important as a new contribution to this study and not only as a contribution to child psychology.

The translation appears to be excellent. It is open to one criticism. The original French of the actual recorded remarks of children should have been given.

R. H. T.

Problems of Personality. (Studies presented to Dr Morton Prince.) Board of Editors: C. MacFie Campbell; H. S. Langfeld; Wm McDougall; A. A. Roback (*Secretary*); and E. W. Taylor. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1925. Pp. xiii + 434. 25*s*.

This volume consists of a collection of twenty-four essays of various lengths and on various topics by twenty-four admirers of Dr Morton Prince.

The essays have been grouped in four parts. Part I contains *The evolution of intelligence and the thraldom of catch phrases* (Prof. Elliot Smith), *Abnormal psychology and social psychology* (Dr Ernest Jones), *Notes on suggestion, empathy and bad thinking in medicine* (Prof. W. A. White); Part II contains *Does the will express the entire personality?* (Prof. Claparède), *An experience during danger and the wider functions of emotion* (Prof. Stratton), *On recent contributions to the study of the personality* (Prof. Campbell), *Character and inhibition* (Dr Roback); Part III contains *On memories which are too real* (Prof. Janet), *Some medico-legal experiences with comments and reflections* (Prof. Mills), *Some medical aspects of witch-craft* (Prof. Taylor), *Divisions of the self- and co-consciousness* (Dr Mitchell), *The handwriting in nervous diseases with special reference to the signatures of William Shakespeare* (Prof. Dana), *The static and kinetic representations of the efferent nervous system in the psycho-motor sphere* (Prof. Ramsay Hunt), *The development of psycho-pathology as a branch of science* (Dr Bernard Hart), *The subconscious, the unconscious and the co-conscious* (Prof. Knight Dunlap), *The association of psycho-neuroses with mental deficiency* (Dr C. S. Myers); Part IV contains *Prof. Freud's group psychology and his theory of suggestion* (Prof. Wm McDougall), *Psychological types* (Dr Jung), *Suggestion and personality* (Dr Wm Brown), *The unconscious in psycho-analysis—a criticism* (Prof. Goddard), *Unconscious dynamics and human behaviour: a glimpse at some inter-relationships of structure and function* (Dr Jelliffe), *The metamorphosis of dreams* (Dr MacCurdy); Part V contains *Conflict and adjustment in art* (Prof. Langfeld), *Prince's 'Neurogram' concept in its historical position* (L. H. Horton).

The essays, both those which set out present-day hypotheses and those which present new material, are stimulating and will be of interest not only to the psychologist but also to that numerous public which wishes to be informed of modern trends in psychology.

Mental and Physical Traits of a Thousand Gifted Children. (Vol. I of *Genetic Studies of Genius.*) By Lewis M. Terman. Stanford University Press. 1925. Pp. xvi + 648. $5.00.

This volume, the first volume of a work with the rather ambitious title of *Genetic Studies of Genius*, presents, in twenty-one chapters, the results of an enquiry into the mental and physical traits of over 1400 children who, when tested with such tests as the National, the Terman Group, and the Stanford-Binet, ranked well within the top 1 per cent. of the unselected school population of the largest cities in California. The greater part of the volume is devoted to a study of 643 of these children.

The aim of the enquiry was to find out in what traits, and to what extent, a representative group of intellectually superior children differs from a group of unselected normal children.

The enquiry has been made with great thoroughness by a large band of workers. Though methods subject to the influence of the personal equation could not be entirely eliminated, the main conclusions are based on well-defined experimental procedure, and, since the experiments are described fully, it is possible for them to be repeated. We are given genealogical studies and vital statistics of the families represented (necessarily not very complete); anthropological measurements; the results of medical examinations; physical and educational histories; tests of school accomplishment, character, and personality; specialization of abilities; interests—scholastic, occupational, play, reading, social; etc.

This mass of information has been well set out, and at the end of each chapter is a complete summary which greatly facilitates reference.

It is recognized that this is the first step only in the investigation, which it is proposed to continue, with the same subjects, for many years. One such "Follow-up," two years after the original enquiry, has already been made and the results are summarized in the twentieth chapter. The whole value of this enquiry depends on the thoroughness and care with which the future work is done.

Prof. Terman and his assistants are to be congratulated on the success with which the first phase has been completed

Modern Psychology and Education. A Text-book of Psychology for Students in Training Colleges and Adult Evening Classes. By E. C. OAKDEN and T. RAYMONT. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1926. Pp. xxiv + 320. 7*s.* 6*d.* net.

This elementary book on psychology and its applications to education is admirably adapted to its aim, which is "to put before students beginning psychology a sketch of the subject which may be comprehensible to them because it deals with common experience illustrated by concrete cases." It is intended, as the sub-title indicates, chiefly for those who are not in a position to devote a large amount of time to the study of psychology. The illustrations are a feature of the book. They are largely drawn from literature, so that the student is trained to apply what he learns to his normal life. This training is furthered by the manner in which the applications to educational matters are treated. and by the numerous suggestive questions at the end of each chapter.

One important factor in education, so often neglected in works of this nature, viz. the teacher and his problems, also receives attention. While the student, who wishes to widen his knowledge by further reading, will be helped considerably by the list of references to other works.

The book contains nothing new; but it meets well the needs of those for whom it is intended.

A Text-book of Experimental Psychology. By C. S. MYERS and F. C. BARTLETT. Part II: Laboratory Exercises. Cambridge University Press. 3rd edition. 1925. Pp. 121. 7*s.* net.

This new edition incorporates the greater part of the contents of the previous issue (1911), but contains some interesting changes in both matter and form. In the second edition the experiments in Part II followed the order of discussion in the text-book (Part I); in the present edition they have been re-arranged and grouped systematically in four sections. Of these, the first deals with experimental technique, the exercises being arranged to illustrate both qualitative and quantitative methods; the second with the special senses; the third with perception and the higher mental processes; and the fourth with further experiments on sensory processes, on time estimation, and on attention and feeling.

The change in matter consists mainly in the addition of a number of valuable experiments, devised by Mr Bartlett, on the processes of imaging and on the higher mental functions of recognition, analysis and construction. These substantial improvements should give a new lease of life to what is still the best available text-book of experimental psychology.

W. H. O'N. M.

The Memory Factor in Biology. By Dr C. J. PATTEN. London: Baillière, Tindall and Cox. 1926. Pp. xiii + 175. 5*s.* net.

This interesting though not very critical study treats Memory as "the Mainspring, the *vis a tergo* of Evolution." Its basis is said to be physical, for the external stimuli upon which it is founded leave permanent traces "in the protoplasm," and indeed pass onwards from one generation to another. Containing nothing particularly new the essay is very well and clearly presented.

Learning how to Study and Work Effectively. By W. F. BOOK. Boston: Ginn & Co. 1926. Pp. xviii + 495. $1.96.

This study of the learning process is divided into five parts. The first deals with *The Problem of Personal Efficiency*; the second with the *Physiological and Psychological Basis for Personal Efficiency in Study and Work*; the third with *Learning how to Study and Work in the most Effective Way*; the fourth with *Application of the Principle of Personal Efficiency to the Performance of Specific Tasks*, and the fifth with the *Dangers of Pseudo Efficiency in Learning to Study and Work.* Obviously the book is intended to make a wide appeal. It is clear, well-presented and engagingly confident. It contains much common sense, much well-attested psychological fact. It could have been condensed with advantage.

A Theory of Direct Realism: and the Relation of Realism to Idealism. By J. E. TURNER, M.A., Ph.D. London: Allen and Unwin. Library of Philosophy. Pp. 324. 12*s.* 6*d.* net.

This book is of philosophical rather than psychological importance. The author presents a realistic theory of external perception (similar to Prof. Alexander's) which, he believes, avoids the pitfalls of subjectivism and noumenalism and shares, in larger measure than any other contemporary doctrine, the strenuous naivete of the plain man's conception. Dr Turner, it seems to us, has not appreciated the nature of some of the difficulties with which he deals, and his solutions are in consequence unconvincing. On its negative side, however, his position rests on firmer ground and affords much pertinent criticism of current realist views. The work, on the whole, is well written and should provide a useful basis for discussion.

The Hand and Disease. By NOEL JAQUIN. London: Cecil Palmer. 1926. Pp. xi + 179. 5*s.* net.

This book purports to study "the human hand and its markings from a scientific and reasonable standpoint."

The Ego and Spiritual Truth. By I. C. ISBYAM. London: The C. W. Daniel Company. 1926. Pp. xiv + 182. 7*s.* 6*d.* net.

The Self Seeker and his Search. By I. C. ISBYAM. London: The C. W. Daniel Company. 1926. Pp. xi + 128. 5*s.* net.

In these two books the author carries further the philosophy which he expounded in *The Ego and Physical Force.*

Medizinische Psychologie. Von Dr Ernst Kretschmer. 3rd edition. Leipzig: Georg Thieme. 1926. S. 273. M. 15.30.—Geb. M. 17.50.

This new edition is very considerably enlarged and brought up-to-date. It is, of course, well written throughout and forms a most interesting, though provocative, introduction both to modern psychopathology and to a great many problems of social life and development.

Theorie des Gedächtnisses. Von Dr Julius Pikler. Leipzig: Johann Ambrosius Barth. 1926. S. 43. M. 1.80.

Prof. Pikler here continues his contribution to the "accommodation theory" of the perceptual processes. His monograph contains two parts, the first dealing with a theory of remembering and the second being a criticism of modern *gestalt* theories with special reference to Kohler's *Physiologische Theorie des Sukzessivvergleichs.*

Das Psychologische Profil und andere experimentelle-psychologische individuale und kollective Methoden zur Prüfung der Psychomechanik bei Erwachsenen und Kindern. Von Prof. G. J. Rossolimo. Halle: Carl Marhold. 1926. S. 139. R.M. 5.80.

Dr Rossolimo here explains the test methods, both individual and group, by which he endeavours to obtain an accurate psychological outline of the make-up of normal and abnormal individuals. He presents also a reasoned discussion of a large mass of his results.

Theoretische Psychologie im Umriss. Von Prof. J. Lindworsky. Leipzig: Johann Ambrosius Barth. 1926. S. vi + 103. R.M. 5.

This book serves as a second edition of the author's well-known *Umrisskizze zu einer theoretischen Psychologie.* It contains some discussion of recent theoretical contributions to psychology, and is a scholarly piece of work, though by no means easy reading.

Anleitung zu psychiatrischen Untersuchungen. Von Prof. Hans Seelert. Leipzig: Georg Thieme. 1926. S. 181. Geb. M. 5.70.

This is a useful and clear though exceedingly abbreviated book for students who are beginning the study of psychopathology. It covers a great amount of ground and is to be recommended provided it is not used entirely to replace more thorough discussions.

Die Lüge bei Kindern und Jugendlichen. Von Dr Franszizka Baumgarten. Leipzig: Johann Ambrosius Barth. 1926. S. 121. R.M. 6.

The author has studied the frequency and motives of lying under home and school conditions, and in its social aspect. She considers the relation of lies to pity or regard for others, and to shame or bashfulness; and discusses the educational significance of her results. A useful bibliography, very weak however on the English side, is added.

NOTES ON RECENT PERIODICALS

Psychologische Forschung.

Vol. VII, No. 3, Feb. 1926.

Experimentelle Untersuchungen uber das Ordnen von Gegenstanden. (Richard Meili.)

Various experiments were carried out upon men and women students to establish rules for the grouping of objects, which were in some ways related but otherwise dissimilar. It was found that colour, shape, purpose, weight, size, all played a part in attempted schemes of classification, but the most satisfactory method appeared to be a grouping upon the basis of the use of the objects.

Die Principielle Lage in der Tierpsychologie. (H. Dexler.)

The selection theory laid the foundation for the structure of animal psychology. Modern 'Gestalt' theories have now shown an important turning-point in its history. Observation has proved early mechanistic views of animals to be mistaken, and a preferable standpoint to be that of psychophysical parallelism. On the other hand, popular animal psychology often shows a tendency in an opposite direction, ascribing to animals all human attributes and affects; whereas the state of affairs lies probably between the two, with a highly complicated reflex and instinctual system forming one end of the scale, and the widely ranging 'Gestalt'-characteristics of perception and sensation taking up their position at the other.

Experimentelle Untersuchungen über simultane und sukzessive Gesichtswahrnehmungen. (Georg Katona.)

The aim of this paper is to ascertain the difference in facility in remembering small designs, shown in pairs, if the two halves are shown simultaneously or in rapid succession. The results of three tests proved recognition of the small figures more readily accomplished when shown in succession.

Vorlaufige Mitteilung uber die Wirkung der Suggestion auf Nachbilder. (Erwin Schroff.)

These tests always seem to ignore the common-sense factor, that children deem it politic to agree with a person who is adult, and say what is required, or, they will be entirely contrary. So that these tests far from really giving evidence upon the effect of suggestion upon the After-Image, show how the children or the investigators themselves react to suggestion, of the two types, autosuggestion and heterosuggestion.

(1) Josef Ternus describes an apparatus for providing acoustic stimuli of quantitative variability; (2) Kurt Lewin an improved apparatus for testing time sense, and (3), the same author, a description of a counting Chronograph.

Archives de Psychologie.

Vol. XIX, No. 75, June 1925.

Psychologie et critique de la connaissance. (Jean Piaget.)

Presents a summary of the history and development of science, with the intention of demonstrating the relationship between psychology and the theory of learning.

La structure des récits et l'interprétation des images de David chez l'enfant. (Emilie Margairaz and Jean Piaget.)

This test was to discover to what extent children are able to reconstruct a story, when they have been shown two pictures simultaneously, not in the right order, one of which represents the beginning and the other the end of the narrative. The child is required to understand the pictures and from them to deduce the intermediate events. The results of the test showed: (1) the incapacity of young children (those tested ranged between four and twelve) to understand that the pictures formed part of the same story with relational sequence, (2) their inability for synthesis in style or thought, and (3) their difficulty in realizing causality.

Doit-on tenir compte des erreurs dans les tests à temps fixe? (Pierre Bovet.)

This experiment was made in connection with the Barcelona test, which consists of 70 miscellaneous questions to be answered in writing in half an hour. It is given to test general intelligence. The writer considers this may be estimated most adequately by the simple method of recording the number of correct answers.

Tests d'Ozeretzky pour développement des fonctions motrices de l'enfant. (Regina Merkin.)

These tests, which form a scale of motor efficiency, were primarily arranged to investigate the development of children at the medico-pedagogical clinic in Moscow; and were again employed in the Maison des Petits and the Primary School of La Roserie, in order to ascertain to what extent the development of these children might be considered to correlate with that of the children of Moscow. Results on the whole proved great similarity of development at corresponding ages.

Vol. XIX, No. 76, Dec. 1925.

Les temps de réaction et la Psychologie Appliquée. (Ed. Claparède.)

Investigation of the value of the reaction time for applied psychology shows, according to some psychologists, that it is of little practical value, since it varies considerably in pathological cases only. It seems to be of more interest to individual psychology, where it depends upon the type of reaction, motor or sensory, but still more upon the innate aptitudes, or actual natural idiosyncrasies of the individual tastes. The most important factors to be taken into consideration, in research connected with reaction times, appear to be actual nerve conductiveness, that is *synaptic function*, or inertia, attention and interest.

Expériences sur la constance des temps de réaction simple. (Lea Fejgin)

This paper contains the result of a research carried out upon an equal number of men and women to test reaction time to auditory and visual stimuli. The results show a remarkable similarity of both kinds in the same individual. On the whole, the men showed greater rapidity than the women. It is to be expected that the subjects tested would react to the affairs of life with the same proportion of rapidity as in these laboratory tests.

Comparaison des réactions simples et discriminatives. (Berthe Kraus.)

The aim of this investigation was to ascertain whether a constant correlation existed between simple and discriminative reactions, or if it were always necessary to subject individuals to separate tests to determine their reactions. Results showed wide variability between the two, but introspective material was not available in sufficient quantity to discover the cause of this divergence in ability.

Contribution à l'étude de la valeur des tests de Binet pour l'examen des enfants arriérés et psychopathes. (Mdlle S. Rabinovitch and Mme Rossolimo-Savitch.)

The conclusions reached are that, whereas the Binet-Simon method is of great value in testing normal children and those educationally backward, on account of its simplicity and rapidity, its drawbacks are that it requires special training on the part of the investigator, is only useful to gain a preliminary orientation, and is not applicable for the examination of developmentally retarded children or psychopaths In the latter case, a special system invented by Madame Rossolimo is considered to give more satisfactory results.

La notion de l'ordre des événements et le test des images en désordre. (Hélène Krafft and Jean Piaget.)

Numerous experiments similar to those described in the June number of the *Archives de Psychologie,* upon children of 7–8 years, prove once more that before this age they find great difficulty in placing pictures, representing consecutive events, in their correct order. The sequence of cause and effect seems too hard for them to grasp or to express in words. Their deductive reasoning is extremely weak. Frequently it was found that the pictures themselves that were shown failed to give the right impression of the occurrence intended, but we doubt whether adults would not have found the same difficulty in some cases, for a few were exceedingly ambiguous, especially if first encountered not in order of correct sequence.

Harvard Monographs in Education.

Series I, Vol. II, No. 2; whole No. 7.

Disability in Reading and its Relation to Personality. (Elizabeth M. Hincks.)

In this monograph an account is given of a study which was carried out with the object of discovering the reasons why fifteen children had difficulty in learning to read, and of the attempts which were made, not without success, to improve the children's reading powers. Full accounts are published of eleven of the children, six boys and five girls, whose ages varied from 6½ to 15 years.

Each child was examined as to Intelligence; Visual acuity; Extent of visual field; Ability to make associations between form-colour, form-sound-colour, form-sound-meaning, and form-meaning; Auditory memory of digits and sentences; Pitch discrimination; Left or right-handedness; Reading ability; Visual word span; Recognition of words as wholes; Eye movements when reading. His history, home life, interests, emotional characteristics, and the occurrence of similar reading disability in other members of the family were also enquired into.

Only one child was below the average in intelligence. Most of them displayed a strong aversion to reading matter. With one exception the children showed various individual difficulties and irregularities in perceiving forms other than words—some had a poor memory for detail, others for the general outlines. Most of the children had a limited peripheral vision and were below the average in pitch discrimination. Five were left-handed; a sixth was doubtfully right-handed.

The evidence indicates that difficulty in reading is hereditary and that it is very closely connected with nervous traits in the individual or his family. All the children showed abnormal personality traits, and the author concludes with the remark that "the light these studies throw on the effect of a mental handicap on the development of character and personality in intelligent children" is "of considerable interest." The reviewer wonders whether the 'mental handicap' and the defects of 'character and personality' are not both produced by the same cause.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 16, 1926. Conative Control, by W. J. MESSER.
Does the psychogalvanic phenomenon indicate emotion? by R. J. BARTLETT.

December 11, 1926. Annual General Meeting.
Practice, Fatigue and Oscillation, by J. C. FLUGEL.

SECTIONAL MEETINGS.

AESTHETICS SECTION.

October 1, 1926. Cadence and Phrase tests in Music, by H. LOWERY.

November 5, 1926. Personality and Art, by WILLIAM BROWN.

December 9, 1926. The Modern problem of Form and Content in Art, by A. H. HANNAY.

EDUCATIONAL SECTION.

October 11, 1926. Mental Tests as a means of Classifying secondary school pupils in Mathematics, by J. BROWN.

November 8, 1926. Sublimation and Education, by F. C. BARTLETT.

December 6, 1926. Verbal Expression and Personality in Children, by M. M. LEWIS.

January 3, 1927. The Cinema in Relation to the Mind of the Child, by S. J. F. PHILPOT and BARBARA LOW.

INDUSTRIAL SECTION.

December 9, 1926. The Attitude of Management to Industrial Psychology, by members of the staff of the National Institute of Industrial Psychology.

MEDICAL SECTION.

October 21, 1926. The Experimental psychology of Handwriting, by ROBERT SAUDEK.

November 23, 1926. Current tendencies in American Criminology, by BERNARD GLÜCK.

December 11, 1926. Address from the Chair.

VOLUME XVII 1927 PART 4

AN EXPERIMENT FOR THE TESTING OF THE HYPOTHESIS OF LAMARCK[1]

BY WILLIAM McDOUGALL.

I HAVE long felt that a long-sustained experiment for the testing of Lamarck's hypothesis is a crying need. In my undergraduate years Weissmannism or Neo-Darwinism was ascendant in the first flush of its success, and I did not fail to appreciate the force of its contentions and the strength of its position. But it seemed to me then (and it still seems to me now) that all the arguments of the Neo-Darwinists were inconclusive. Support for the Neo-Darwinist position was claimed in a few ill-conceived experiments, such as that which consisted in cutting off the tails of mice for a number of generations. But the repudiation of Lamarckian transmission really rested on the following argument: we cannot understand how Lamarckian transmission can take place, therefore it does not take place. The major premise of this argument remained implicit; explicitly stated, it runs as follows: Only that can happen in nature which we can understand in terms of the mechanistic principles now predominant in the scientific world. Any such premise is of doubtful validity in the physical sciences; and in the biological sciences it is in the last degree foolish and unwarranted. If we seriously accepted it, we should have to deny the facts of heredity *in toto* (as, indeed, in America some of the extremer mechanists seem inclined to do), for nothing is more certain than that we can give at the present time no mechanistic explanation of any of the facts of heredity.

In the third of a century which has elapsed since the time I speak of, the rise of Mendelism has generally been regarded as affording further and strong support to the Neo-Darwinist position. But in reality there is no essential incompatibility between even a most thorough-going Mendelism and Lamarckian transmission. No other facts or arguments of weight against the Lamarckian hypothesis have appeared. On the other hand, a number of instances, experimental or spontaneous, which seem to justify that hypothesis have been brought to light. Of these the results claimed by the late Dr Kamerer have seemed the most striking; and (provided that his accounts were complete and trustworthy) they would seem to establish the truth of the Lamarckian principle. The reception of the work of that unfortunate man by the majority of biologists shows that to support Lamarckism is hardly less dangerous to the reputation of a man of science than to take an active interest in psychical research. But the principal consideration which has seemed to me to demand an open mind upon the Lamarckian hypothesis, and which has impelled me to undertake a most laborious and protracted experiment designed to test its validity, is the following: If the Lamarckian hypothesis be valid, if it be true that modifications of function and structure, acquired by the individual organism in consequence of its efforts to adapt itself to its environment, may be in some degree, however slight, transmitted to its descendants, then we have in outline an adequate theory of organic evolution; and, further, we are able to assign to mind, or, in other words, to purposive, teleological, or hormic activity, an intelligible and leading rôle in the drama. On the other hand, if we can find no warrant for accepting the Lamarckian hypothesis, then we remain utterly in the dark as to the nature of the evolutionary process; we have no theory of it, and the place of mind in the scheme of nature remains utterly unintelligible. For it is now generally admitted that the Darwinian principle of natural selection (including under that term all varieties of selection, organic, sexual, social, germinal, etc.) does not give us a tenable theory of evolution, so long as we postulate only fluctuating variations or indeterminate mutations. And, if we seek to lessen the difficulties of the theory of selection by postulating, as the raw material upon which selection works, mutations that are so markedly adaptive as to have survival value, we cannot assume such mutations to arise by the fortuitous interplay of atoms and molecules. And this difficulty will be especially great when we consider the case of the higher species of animals; for these are capable of compensating very adequately for a multitude of inborn defects by their individual efforts to adapt themselves; hence, among them, only highly adaptive mutations would give their bearers appreciable advantages in the struggle for existence.

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[1] The substance of this article was communicated in a very condensed form to the Psychological Section of the British Association at Oxford in July 1926.

An experimental test of the Lamarckian hypothesis should, it seemed to me, satisfy two conditions, to which, to the best of my knowledge, none of the experiments hitherto made had conformed: first, the adaptation investigated should be one achieved by the intelligent purposive efforts of the organism concerned. Secondly, it should be of such a nature that slight degrees of transmission of the adaptation should be measurable; for the arguments and evidences of the opponents of Lamarck have at least shown that individually acquired adaptations are not transmitted as perfected wholes of structure or function; but that, rather, any Lamarckian transmission (if it occurs at all) must take in the offspring the form of some very slight increase of facility in the acquisition of the modification acquired by the parent; so that only by the cumulation of such effects through many generations will an obvious or easily appreciable change in the constitution of the race be effected.

The last consideration points to a third condition which the experiment should satisfy. It should be made with some quickly breeding species and carried on over a long period of time, *i.e.* through many generations, in order that, if any Lamarckian transmission occurs, it may become more and more clearly manifested in successive generations; or, if no such transmission occurs, the fact may be demonstrated by the absence after many generations of any slightest increase of facility in acquiring the modification in question. Only a well-devised and long-sustained experiment of this kind could afford satisfactory evidence of the invalidity of the Lamarckian hypothesis. And a negative issue of such a sustained experiment would be only of less importance than a positive one. For it would for the first time give us sufficient ground for putting aside the Lamarckian hypothesis as a highly improbable one.

When, in the year 1920, I migrated to Harvard University, I found at my disposal for the first time facilities for carrying on such an experiment, namely a well-equipped though small animal-room in the psychological laboratory. I at once put on foot an experiment of the kind which I had long desired to see instituted and have carried it on continuously from that date. The present article is of the nature of an interim report; for the experiment is still going forward and may, I hope, be continued for a number of years. Two considerations seem to justify the making of such an interim report: first, I desire to reap the advantage of criticism from my colleagues; secondly, I hope that my report may make it seem to others worth while to undertake similar experiments. For the conclusions for or against the Lamarckian hypothesis to be drawn from such experiments are of so high importance for biology, psychology, and philosophy in general as to justify the expenditure upon them of much labour.

Throughout the course of the experiment I have been ably assisted by a number of graduate students to whom I wish to express my hearty thanks for faithful co-operation without which I could not have carried on the work[1].

Professor Pavlov's Experiment.

When our experiment had been in hand for some three years and we were in process of training the 9th generation of animals, Professor Pavlov made his sensational announcement of a successful demonstration of Lamarckian transmission in mice. All available reports were very brief and lacking in detail. But it was obvious that, if Pavlov's work was acceptable, my experiment was rendered nugatory. For Pavlov had adopted a plan of experiment somewhat similar to mine; and he claimed to have obtained in 5 generations results so extremely marked as to establish beyond all doubt the reality of Lamarckian transmission; whereas, after 8 generations of training, my animals showed only slight indications of such transmission. However, I decided to persevere with my experiment because for three reasons I felt sure that Pavlov's statements were founded on some very serious error. First, if Lamarckian transmission occurred in nature with the strength reported by Pavlov, it would be incredible that we should not already have knowledge of many unmistakable instances of it. Secondly, my own experiments, extending over 8 generations, seemed to show that any Lamarckian transmission, if it occurs, must be very much slighter in degree than the startling results claimed by Pavlov would imply. Thirdly, the brief statements by Pavlov, which alone were accessible to me and which, I believe, were all that he has hitherto published on this topic, did not make clear to me the details of his procedure. But when I attempted to plan a similar experiment, I found it by no means easy to devise a satisfactory procedure, one which should make it possible to measure slighter degrees of increase of facility in mastering the task imposed. Throughout a period of two years, assisted by Mr A. R. Stone, I endeavoured to work out a satisfactory procedure along the lines indicated by Pavlov, but found the difficulties insuperable. Pavlov's procedure had consisted in training white mice to run to their food-trough at the sound of a bell. According to his statement, the first generation required about one hundred repetitions of the ringing of the bell in order to learn to respond immediately to it; whereas the fifth generation required only some five repetitions of the "conditioning process." To anyone who has not attempted to train animals in some such way, it might seem that the procedure indicated presents no difficulties. But questions present themselves. In the course of training, how are the animals to be brought into touch with the food? Are they to be brought to the food, or is the food to be brought to them, etc.? In my attempt to repeat Pavlov's experiment, we used white rats, which I have found to be more intelligent, more hardy, and in every way more satisfactory than mice. And, instead of food and hunger, we made use of water and thirst. For we have found thirst to be a more urgent impulse than hunger and one more easily regulated to an approximately standard degree. There was no difficulty in training the rats to run to their water-trough at the sound of the bell, which always coincided in time with the falling of water into the trough. But we did find a difficulty, one which hitherto we have found insuperable, when we attempted to measure in any way the facility of acquiring this response; and at present I am disposed to believe that this difficulty must remain insuperable.

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[1] It seems proper to mention by name several of these students to whom my indebtedness is very great. Mr E. C. Heck participated in the work during the first three years and is now again giving me his highly appreciated assistance. The late Amelia Brackett worked with quite extraordinary devotion and assiduity during the three years preceding her untimely death in the summer of 1926. Mr Cuthbert Clarke was especially helpful in planning and constructing our apparatus. I have also to thank the Corporation of Harvard University for a grant from the Milton Fund which from 1924 onward has greatly facilitated the work.

I therefore wrote to Professor Pavlov, begging him to inform me a little upon the present state of his experiments, since I was interested in repeating them. He most courteously replied, briefly stating that he no longer held his deductions from his experiments to be valid. I was therefore very glad, on visiting Cambridge (England) during the summer of 1926, to find there Dr Anrep, who is in close touch with Professor Pavlov and has in hand a translation of all his more important lectures on "the conditioned reflex." Dr Anrep was so kind as to inform me that Pavlov has authorized him to insert in the volume of translated lectures a note to the effect that his various statements upon the hereditary transmission of "the conditioned reflex" are revoked entirely, serious defects having been discovered in the process by which the positive conclusions had been reached; and Dr Anrep, since the date of publication of his translation is uncertain (though probably an early one), gave me permission to mention in this paper the facts as here set forth.

The publication and revocation of Pavlov's claim to have proved Lamarckian transmission, by a procedure somewhat similar to my own, is, from my point of view, very unfortunate. Pavlov's claim has excited world-wide attention and has already been given to the public in various popular books. It must, therefore, seem to take the wind out of my sails and to deprive of all originality and priority the results to be reported in this article. But more serious is another inevitable consequence. English, American, and German biologists have long been disposed to regard with scornful intolerance all enquiry into the Lamarckian problem. (In France, the native land of Lamarck, on the contrary, it would seem that a majority of biologists have continued to be Lamarckians, an instructive illustration of the influence of non-rational factors in science.) Pavlov's claim and its subsequent revocation can only have the effect of hardening this scepticism on the part of so many biologists. They will hardly find patience to consider any such evidence as is here presented; but rather will say: There goes yet another unfortunate fanatic chasing a will-o'-the-wisp which has misled so many. Let us hope that he will retain sufficient honesty and sanity to discover his error and to avow it as frankly as Pavlov has done.

I will, therefore, make here very emphatically the following statement: I do not claim that the experiment here reported establishes the validity of the Lamarckian principle; I claim merely that the results hitherto obtained point towards such a conclusion and justify further experiment along similar lines. I am well aware of the possibilities of many subtle flaws in my procedures and of subtle errors in the reasoning process. Hitherto I have failed to discover such flaws or errors; and I may add that, on the occasion of my brief verbal presentation of this work before Section J of the British Association in August 1926, none of the biologists then present succeeded in pointing out any serious flaw or error—or so it seemed to me. I will add one comment on Pavlov's work on this problem. Pavlov is a pronounced mechanist and is what in America is barbarously called a "Behaviourist"; that is to say, he will have nothing to do with psychology, but insists on regarding the actions of animals as a series of mechanical reflexes, inborn and acquired or "conditioned." The history of his attack upon this problem by strictly "behaviouristic" methods seems to me to illustrate the truth of a proposition which I have insisted upon elsewhere[1], namely, that delicate problems of animal psychology cannot be profitably investigated by the "behaviourist" or by behaviouristic methods. In face of them such methods are sterile and misleading. They can only be profitably investigated by psychologists using the methods of psychology.

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[1] "Men or Robots" Two lectures published in the volume of Powell lectures of Clark University entitled *Psychologies of 1926*.

The Animals used.

After due consideration, I chose, as the material for my experiment, white rats; the principal considerations in their favour being (1) ease of care and handling, (2) rapidity of breeding, (3) relatively high intelligence or power of individual adaptation of action to novel situations, (4) the possibility of obtaining pure-bred standard stock. Experience has amply justified the choice in respect of all these points. Under the first head I include the hardy nature of the animals. I obtained as initial stock from the Wistar Institute of Philadelphia 6 females and 3 males, all young, of their standard stock which has long been carefully inbred. They have proved extremely satisfactory. On a mixed diet, including a little dried milk and fresh green vegetables (especially lettuce) but of which the foundation has been bread and dog-biscuit, they have thriven and bred freely. I have lost a few through excessive heat in exceptionally hot summer weather. They seem unable to bear a temperature of the air at or near 90° F.; and, since their room is beneath a skylight, a few casualties have been incurred. Deaths and cases of sickness from other causes have been very few among the many hundreds of animals we have raised. A large number have been subjected to daily immersion in cold water: but not one animal has shown any ill-effects. Immersion was never prolonged beyond three minutes. After immersion in water below 70° F. for that time they look miserable and their vitality seems depressed; but they quickly recover and, after their coats are dried (which is effected in some fifteen or twenty minutes), they seem perfectly normal. I have the impression that their health is all the better for the daily bath.

My plan was to divide an original stock into two halves which should be as nearly as possible alike in respect of genetic composition, vigour, and all other qualities; to train successive generations of one half-stock in the acquisition of facility in one task, and the other half-stock in acquisition of facility in another and quite different task; then, using animals of each half-stock as controls, to look in successive generations for any increase of rate of acquisition of facility in respect of the task to which they and their forbears were trained.

It soon appeared that there are very large individual differences between the animals in respect of the rate at which they master their task or acquire facility in it, and that there are similar differences between strains or families. This fact renders it very necessary to exclude with care any selection of animals that might interfere with the significance of any apparent increase of facility of learning or rate of acquisition. Some selection was inevitable, because it was, of course, impossible to train all the rats born in each generation. We were able to train only a limited number of each generation to each of the two tasks; and could use for breeding purposes only a still more limited number of animals. The number trained in each generation varied according as we did or did not design to make in that generation control experiments. The animals to be used for training and breeding were selected *at random* from the young litters before training of them began, generally two from each litter; the only selection, other than random, being the rejection of any obviously weakly individual; runts occur not infrequently. Litters of 12 or more are common; our usual practice was to destroy within the first week after birth all but 6 members of each litter; for, if the whole of a large litter is reared, the individuals do not thrive as well as in smaller litters. In a few cases in which this random selection of young animals was not effected before training began, we chose for breeding the most and the least successful member of each litter trained. *In these ways we avoided, I think, any selection of a kind that would vitiate our results.*

The 9 rats forming our original stock were allowed to breed, one male to each two females. Six litters were produced, and we divided the total number of young rats into two equal half-stocks by taking at random from each litter one-half of its members and placing them to the one half-stock, and the other half of its members to the other half-stock. Thereafter each half-stock was strictly inbred. To avoid any confusion and consequent crossing of the two half-stocks, from all members of the one (henceforth called half-stock T) we snipped off the tip of the left ear; and similarly from all the members of the other half-stock (henceforth called half-stock B) we snipped off the tip of the right ear. For the purpose of distinguishing individuals we used a system of marking the coats with chromic acid. We found this very satisfactory; the acid, applied in solution with a brush, forms a bright yellow stain which endures for several months.

Choice of Tasks.

The choice of the two tasks in the performance of which the two half-stocks were to be trained was obviously a matter for careful consideration and experiment. As is illustrated by the foregoing reference to Professor Pavlov's attempt in this direction, it is easy to choose an unsuitable task. The possibility of delicate measurement of degrees of facility in learning the task seemed to me the first and all-important desideratum. I propose to describe first the procedure adopted and results obtained with the half-stock T; and, as the process of arriving at the simple procedure finally adopted was a long one and very instructive in several respects (it was not finally perfected until after work on 9 generations of rats) it seems worth while to describe it briefly.

My first notion was to train the rats to run in a hollow wheel like a squirrel-cage and, by inserting an electrified section in the wall, to train them to leap across this section at each revolution of the wheel. A narrow hollow wheel, 30 inches in diameter, was constructed. Its peripheral wall, on which the rat ran, was of wire gauze; and one section of it, 3 inches wide and visually indicated, was so electrified that the rat, on stepping on it, received through his feet an interrupted current from a secondary coil. The wheel was driven by a motor at a constant speed such as made it easy for the rat to maintain his position at or near the lowest point of the moving surface on which he ran.

With the 1st generation of the half-stock T, it proved easy to induce all the rats to run this wheel smoothly and regularly and to learn to leap very regularly and prettily across the electrified section, thus avoiding the shock; and it seemed to me that I had struck upon an excellent procedure for my purpose. But things did not go so well with the 2nd generation. Instead of any improvement in the rate of learning the task of regular running and leaping, the time required for teaching them was, on the average, somewhat longer than with the 1st generation. The time of learning for each animal was measured in terms of the sum of the periods spent in the revolving wheel; each rat spending in the wheel each day a period that was regulated according to the prescribed schedule. With the 3rd generation the difficulty was greater, the time of learning was, on the average, still longer, and my patience was sorely tried. With the 4th generation, things went still worse and I was baffled. But light broke on the darkness. It had become obvious that the chief difficulty in teaching this task was the tendency of many of the rats to avoid the shock, not by leaping across the electrified section, but by dashing up the side-wall of the wheel (when confronted by the electrified section) and then clambering upon it. The side-walls were constructed of panels of wire gauze and, in order to counteract this tendency, I connected the panels in such a way that in passing from one to another the rat received the interrupted current. But in spite of this corrective procedure, which in the successive generations I had to employ with increasing frequency, the process of teaching the rats to run and leap regularly became in the 4th generation extremely prolonged and tedious.

I began to realize that I was obtaining evidence of a Lamarckian transmission, but not of the kind I had set out to induce; namely, it seemed that the rats were acquiring in successive generations an increasing tendency to avoid the electrified section of the floor, not by leaping across it, but by turning aside from it and dashing up the side-wall. They seemed, in short, to be in process of acquiring what might be called an hereditary aversion or fear, or a phobia, which was brought into play by visual confrontation with the electrified section.

It is perhaps worth while to point out that the observed facts seemed readily interpretable psychologically, but not "behaviouristically." The rat, before he could learn to leap across the electrified section, had to run across it many times, receiving the shock through his feet. The first effect of the shock was, in nearly all cases, to cause the rat to accelerate his movements, to run more rapidly for a moment. According to the principles of "behaviourism," repetition of the shocks at each revolution of the wheel should have confirmed this mode of reaction; and, the coincidence of the visual stimulus with the electric stimulus and its antecedence of the same should merely have led to the evocation of the accelerated movements at a moment preceding the reception of the electric shock. But in no case was this order of events followed. Those rats which quickly learnt to leap across the section commonly, as the first indication of the influence of the visual presentation of the section, paused a moment as they approached it and then leapt across it. Others paused, turned aside and dashed up the side-wall. Many behaved sometimes in the one way, sometimes in the other. If now we make the not very extravagant psychological assumption that the shock startled the rat, that it evolved in him fear with its aversive impulse, the facts observed are readily interpreted. After some repetitions of the shock the visual presentation of the section provokes an anticipatory fear[1]; under this impulse the rat seeks some way of avoiding the contact with the section; only two ways are open to him; he hesitates a moment and then takes sometimes the one way, sometimes the other.

But the point of primary interest to me was that, in the rats of successive generations, the fear-reaction seemed to be evoked more and more readily. In the 3rd and 4th generations the more bold and vigorous rats continued to leap across the section; the more timid ones refused to face it, and persistently turned aside to clamber on the side-wall. There seemed to be clear evidence of a progressive change in the successive generations; it might be that the change was of the nature of a general increase of timidity, or, in other words, an increased sensitivity or ease of excitability of the fear-instinct. Or it might be that the increased excitability or sensitivity was in the receptive process or disposition, that concerned in the visual perception of the electrified section, so that the increased timidity was specific to the particular situation. Either assumption would meet the facts.

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[1] Unmistakable evidence of more or less intelligent anticipation of this kind is recorded in my article, "Instinct and Intelligence in Rats and Cats," in the *Journal of Animal Psychology,* in which also other points of psychological interest arising out of this experiment are recorded.

In planning the experiment, I had, under the influence of the current overstressing of the rôle of habit, thought only of the motor habits that might be set up. I had anticipated the formation of a well-formed habit of leaping across the section at each revolution of the wheel, and the possibility of the transmission in some degree of this habit to successive generations. But I had thought too "behaviouristically"; the transmitted change (if such there really was) was of the nature of increase of timidity, either general or specific to the situation.

In so far as there might be any transmission of the motor habit of leaping across the section, this must work against the other tendency to avoid the section by clambering on the side-wall. My procedure was, then, badly planned; it was one that produced a twofold effect in the rats, two incompatible tendencies, and, if Lamarckian transmission is at all possible, both tendencies might be transmitted and might obscure one another. It was necessary, therefore, to abandon this procedure or radically modify it. And, since I had obtained indications of the formation of an hereditary phobia, I decided to plan an experiment with the design of developing such a phobia in a pure form.

The Tank-procedure.

I obtained a large tank of galvanized iron, about 40 inches in length, 20 in breadth, and 20 in depth (cf. Fig. 1). Into this was placed a wooden framework consisting essentially of the following parts: (1) two vertical pieces or septa which divided the tank longitudinally into three equal, long, parallel chambers. In the middle of each septum was a wide doorway (5 inches) allowing free access from one chamber to another. At one end, 12 inches above the floor of the tank, was a transverse, horizontal platform *P*. Water stood in the tank to a depth of 6 inches; and, in each lateral chamber, a gangway sloped at an angle of 45° from the platform to the surface of the water. These gangways were constructed of ground glass (resting on brass supports) covered with wire gauze. The design was to place the rat in the water in the middle chamber, in the expectation that he would find his way out of the water by one of the two gangways in the lateral chambers. But his task was complicated in the following manner. One of the gangways (*B* in Fig. 1) was wired in such a way that the rat in the water, on making contact with it, received an interrupted faradic current which traversed him so long as any part of him was immersed in the water. The rats, it should be remarked, invariably exhibit an urgent striving to escape from the water, seeking and exploring every possible means of exit.

At first the procedure consisted merely in placing each rat in the middle chamber (at the spot *O* in Fig. 1) again and again (six times each day), leaving him each time to find his way out of the water by one of the only two possible routes, the two gangways *B* and *A*. Under this procedure all rats soon learnt to avoid gangway *B* (the electrified one) and to escape quickly from the water by means of gangway *A*. It must be added that gangway *B* was made visually distinguishable from *A* by a strong glow-lamp which shed its light from behind the glass gang-way *B*, through it and along the chamber. Since the whole of the rest of the tank was only dimly illuminated, the visual warning-signal thus provided was very distinct to human vision. The white rat is supposed to have very poor vision and to make but little use of it. But there was ample evidence that the rats quickly learnt to re-act to this visual signal. Since his retina is said to be more sensitive to green light than to light of other wave lengths, a strip of green glass was laid along the middle of the illuminated gangway. This was discarded in the latter part of the experiment, as it seemed to be of no value.

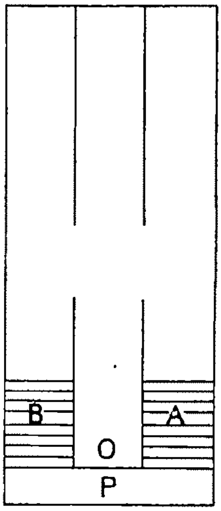

Fig. 1.

Each rat learnt quickly to avoid *B* and to take invariably the route *A*; he learnt this after a small number of shocks on *B*, *i.e.* after receiving shocks a number of times which seldom exceeded 20. And some of the rats, having from the first taken (by happy chance, one might say) the route *A* and adhered to it, received no shocks at all. In order, therefore, to secure that each rat should suffer the shock a considerable number of times, a sliding door was placed in the doorway leading from *O* to *A*. When the door was closed, its lower edge was 3 inches below the surface of the water. The door was kept closed for one minute after the immersion of each rat; and then, if the rat was still in the water, the door was raised well above the water-level, giving free egress to *A*. Under this modified procedure, each rat took the route *B* a large number of times; but each one learnt eventually to avoid *B* by diving under the door blocking the way to *A*.

I seemed now to have a satisfactory procedure, and I trained 7 generations of rats to escape under these conditions. Two methods of testing, of discovering any trace of Lamarckian transmission of the effects of training, were possible. One method was to record for each rat the number of times he took the route *B* and received the shock, before learning to avoid *B* and escape always by *A*. The other method was to test each rat before his training was begun, in such a way as to reveal any inborn tendency to avoid route *B* and prefer *A*. When I had persevered with this procedure up to this point, I discovered in it a flaw which may have invalidated the positive conclusion to which it seemed to point. I therefore altered the procedure for the training of later generations; and since this flaw makes it impossible to attach decisive importance to the results obtained up to this point, I shall present here only the results of the testing of the 8th generation by the second method.

Testing of Eighth Generation in the Tank.

The test was applied in the following manner. Each rat was taken from his nest-box for the first time during the fourth week after birth, *i.e.* when between 21 and 28 days of age[1]. Holding him by the tip of his tail, I dropped him gently into the water at *O*, his nose towards the platform end of the tank; the conditions in the tank being as follows: the lamp behind the glass gangway *B* was shining through the glass, illuminating the *B* passage; the current was shut off from the wire on the gangway *B*; the door was removed from the doorway between *O* and *A*. The two routes, *B* and *A*, were thus equally open to the rat, the only difference between them being in the bright illumination of route *B* and the dim illumination of route *A*. The rat invariably found his way out, in the course of a period that seldom exceeded a hundred seconds, by one of the two routes, *A* and *B*. After one minute's rest, he was immersed again in the same way, and again he escaped. Each rat was immersed in this way six times on the first day on which he was taken from his nest-box; and the number of times he escaped by route *A* and route *B* was recorded.

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[1] Three weeks is the earliest age at which the rat becomes capable of vigorous running and swimming, and, therefore, at which testing or training can be begun.

In this way I tested the rats of the 8th generation, *i.e.* the 8th generation from the adoption of the Tank-procedure. More exactly, having subjected the first 4 generations of the half-stock T to the procedure in the wheel, I began the Tank-procedure with the 5th generation of that half-stock, and shall speak of this as the 1st generation of Tank-rats. The training of this 1st generation of Tank-rats was somewhat irregular and slight. Then followed generations 2 to 6, of which the training was thorough. The 7th generation, owing to an unfortunate delay in breeding, which seemed to be due to a deficiency of vitamine in the diet (completely corrected by a more copious supply of lettuce), escaped all training. The 8th generation of Tank-rats, the testing of which is here reported, thus followed upon a generation that had remained untrained, *i.e.* had not been subjected to the Tank-procedure.

It was impossible to foresee whether the difference of illumination between the two routes *A* and *B* would determine normal rats (that is, rats whose ancestors had not been trained in the tank) to prefer one route to the other. It was, therefore, necessary to compare the behaviour in this respect of the Tank-rats with that of normal or control rats. For this purpose I used 35 young rats of the other half-stock B, subjecting them at the same age to exactly the same procedure, each one to six immersions. These 35 control rats, taken from 6 litters, gave, in a total of 210 testings, the following results:

TABLE I. 210 *Testings of* 35 *Control Rats.*

Dim *A* route taken 85 times.
Bright *B* route taken 125 times.

The 26 Tank-rats, taken from seven litters, gave in a total of 156 testings the following results:

TABLE II. 156 *Testings of* 26 *Tank-Rats.*

Dim *A* route taken 84 times.
Bright *B* route taken 72 times.

From these figures it appears that the normal or control rats are to some extent attracted towards the bright route *B*; and that the Tank-rats, whose ancestors during 6 generations have (through repeated shocking on the gangway *B*) learnt to escape by way of *A* and to avoid *B* entirely, are to some extent repelled by the bright route *B*.

The difference between the figures is large enough to be significant, I think. But no sure conclusion in favour of the Lamarckian principle can be drawn, for the following reason: I observed that, while the members of most litters would go pretty evenly to both *A* and *B* before training, all members of some litters and some members of others showed a strong bias to one side or the other, *e.g.* in one extreme case, every member of one litter of control rats took, on each of their six testing occasions, the route *A*. It seemed that what might be vaguely called a right-handed or a left-handed tendency, or a tendency towards or away from the light, was innate in some strains, quite apart from any training of their ancestry. It was, then, just possible, although extremely improbable, that the control stock happened to contain an undue proportion of strains whose constitution carried such a bias towards the bright side *B*, and that the difference between the Tank-rats and the control rats (in respect of preference for routes *A* and *B*) was wholly due to such difference of innate constitution of the two half-stocks, B and T, obtaining already before my experiment had begun; rather than to a difference of constitution induced by the training of six generations of ancestors to the avoidance of *B*. It was necessary, therefore, to modify the procedure; and this was done in the following way.

The Modified or Alternating Tank-procedure.

The wire gauze covering both gangways, *B* and *A*, was wired so that on turning a switch the interrupted current could be sent through either one of them. Behind each was a glow lamp; and by turning a switch, either lamp could be caused to glow. The procedure now adopted was to switch both currents (that to the gangway and that to the lamp) alternately to left and right at successive immersions of each rat, the two currents always being switched to the same side, *A* or *B*, so that the shock and the illumination always should go together in the experience of the rat under training.

Under this procedure the chances of obtaining a positive result seemed less good than under the former. For, under that earlier procedure, two coincident tendencies became impressed on each rat during training; namely, the motor habit or tendency, on swimming from *O* towards the middle of the tank, to turn rightwards through the door leading to *A*, and at the same time the aversion from *B* generated by repeated shocks on *B*. Under the new procedure the former of these tendencies, the motor habit, could not be formed, but rather only the aversion from the brightly illuminated chamber, which now lay sometimes on the left, sometimes on the right. But, should any difference of behaviour, in respect of taking the Bright or Dim route, appear between the Tank-rats and the controls, that difference would be of more positive significance. Yet it might still be that the two half-stocks had differed from the first in respect of preference for light and darkness. Therefore it would be necessary to rely for the comparison, not so much upon any difference revealed at the testing which precedes the training, but, rather, chiefly upon the rate at which training influenced the two half-stocks, the Tank-stock and the control stock.

The new procedure had a further advantage, namely, that it imposed on the rats a more difficult task, one which made a much higher demand on their intelligence, than did the former procedure, and thus involved them in many more repetitions of the shock upon the brightly lit gangway. Some of the rats required as many as 330 immersions, involving approximately half that number of shocks, before they learnt to avoid the Bright gangway. The process of learning was in all cases one which suddenly reached a critical point. For a long time the animal would show clear evidence of aversion from the Bright gangway, frequently hesitating before it, turning back from it, or taking it with a desperate rush; but, not having grasped the simple relation of constant correlation between bright light and shock, he would continue to take the Bright route as often or nearly as often as the other. Then, at last, would come a point in the training at which he would, if he found himself facing the bright light, definitely and decisively turn about, seek the other passage, and quietly climb out by the Dim gangway. After attaining this point no animal made the error of again taking the Bright gangway, or only in very rare instances. I would test the strength of the acquired aversion by taking the rat by the tip of his tail and dropping him gently into the water, facing and close to the brightly lit gangway. On this, the close proximity of the seen gangway seemed to evoke in the animal a stronger tendency to immediate escape; and, in the case of nearly every rat, he would, sometimes after a short hesitation, take the Bright gangway and receive the shock. But, after a few repetitions of this procedure, every rat absolutely refused to be thus misled, and immediately turned about and took the long route to the Dim gangway—a convincing demonstration that the aversion was evoked visually, *i.e.* by the vision of the Bright gangway.

It is to be noted that the training given under this new procedure may be regarded as continuous with that of the preceding generations under the old procedure; for both procedures generated in the rats aversion (a specific fear or phobia) for the Bright gangway; the difference being that the later procedure generated it in a purer form, *i.e.* uncomplicated by a motor habit of turning to one side and taking always the doorway on that side.

The new procedure was begun in the course of training of the 9th generation of Tank-rats, and has been carried through with 4 generations, the 10th, 11th, 12th, and 13th. It is being carried on with later generations, but here I shall report only the results obtained up to the 13th generation.

Condensed Statement of Results obtained with Tank-procedure.

Results obtained by the method of testing each rat before beginning of training, each rat being tested (cf. p. 279) six times in his fourth week after birth.

TABLE III.

			Dim gangway*	Bright gangway
	29	Tank-rats of 10th generation take	101 times	73 times
	41	„ „ 11th „ „	122 „	124 „
	16	„ „ 12th „ „	55 „	41 „
	23	„ „ 13th „ „	76 „	62 „
Totals	109	„ „ 10th, 11th, 12th, 13th generations take	354 „	300 „

* It is to be remembered that the Dim gangway was always without the shocking current; the Bright gangway was also without the shocking current during these testings, but during training always had the shocking current.

TABLE IV. *Control Rats*†.

	Dim gangway	Bright gangway
87 Control rats take	252 times	270 times

† It seems legitimate to add, to the group of 87 control rats tested by the later or alternating procedure, the group of 35 control rats tested in the earlier stage of the experiment when the non-alternating procedure was used, *i.e.* when the Bright gangway was always on the same side. We have then:

TABLE V.

	Dim gangway	Bright gangway
122 Control rats take	337 times	395 times

Of these 87 control rats, 60 were from the later generations of the half-stock B, while 17 were from litters born by rats newly obtained from the Wistar Institute. It seemed to me that the rats from the half-stock B were the best controls; for, not only had they been selected in such a way (cf. p. 274) as to insure that the genetic constitution of this half-stock B was as nearly as possible the same as that of the Tank-rats, but also the two half-stocks, B and T, had been subjected, from the date of their separation in 1920, to exactly similar conditions, except for the training of them to different tasks; *e.g.* their diet, their physical surroundings, the general handling of them, the degree of inbreeding, all such factors were as nearly as possible identical for the two half-stocks, B and T, throughout the six years of experiment. But, in order to have a further control made with rats not subjected to these conditions, a new stock of 6 females and 3 males was obtained early in 1926 from the Wistar Institute. These were descended from the same stock as my original stock of 1920. The results obtained in the testing of these two kinds of control rats are amalgamated in the table above, because there was no significant difference between them. No controls were used in the testing of the 11th generation. But in testing the 10th, 12th, and 13th generations of Tank-rats, controls were used (41, 7 and 39 respectively) and the testing of the control rats was carried out under exactly the same conditions as that of the Tank-rats. In order to secure the closest similarity of conditions, I would on the same occasion test two or three Tank-rats, then two or three control rats, then another batch of Tank-rats, and so on.

On comparing the two tables (Tables III and IV) it is to be noted that, while the control rats showed a slight predominance of the Bright over the Dim gangway, in respect to the total number of times each was taken, the Tank-rats showed a more decided predominance of the Dim over the Bright gangway. The difference is not very large; but in view of the large number of rats tested and the total number of tests, amounting to 654 for the Tank-rats and 522 for the control rats, I venture to think it is significant. *And I am unable to account for it in any other way than by assuming that in the Tank-rats the aversion from the Bright gangway, induced in the successive generations by the training process, had become in some degree innate in the later generations.*

Results obtained by Comparing Tank-Rats with Control Rats in respect of Rate of Acquisition of Effective Avoidance of the Shock.

I report here the results obtained with 23 Tank-rats of the 13th generation and with 39 control rats. Of the latter group, 22 were of the half-stock B, and 17 were from the stock newly obtained from the Wistar Institute in 1926. I present first a table showing in parallel columns the figures representing the total performance (*i.e.* amalgamated figures) of the 23 Tank-rats and that of the 39 control rats, on each day of training up to the 30th day, each rat having been immersed and having escaped from the water six times on each day. The 30 days of training were not strictly successive, *i.e.* occasionally, through pressure of work, a day intervened when no training was carried on; but this irregularity was the same for both batches of rats.

TABLE VI.

	23 Tank-rats, 13th generation		39 Control rats	
Day of training	Dim gangway	Bright gangway	Dim gangway	Bright gangway
1st	62	76	99	135
2nd	66	72	102	132
3rd	74	64	112	122
4th	68	70	109	125
5th	71	67	119	115
6th	68	70	102	132
7th	69	69	119	115
8th	68	70	119	115
9th	67	71	111	123
10th	67	71	111	123
11th	72	66	108	126
12th	79	59	110	124
13th	80	58	113	121
14th	81	57	117	117
15th	78	60	111	123
16th	75	63	109	125
17th	80	58	108	126
18th	89	49	112	122
19th	88	50	107	127
20th	97	41	118	116
21st	98	40	123	111
22nd	97	41	117	117
23rd	101	37	108	126
24th	107	31	122	112
25th	105	33	118	116
26th	111	27	122	112
27th	111	27	124	110
28th	116	22	117	117
29th	119	19	121	113
30th	115	23	126	108

The training of the Tank-rats was not carried beyond the 30th day, because on or before that day all, except three, of the Tank-rats had learnt to avoid the Bright gangway and to escape always (or with very rare exceptions) by the Dim gangway.

The notable features of the foregoing Table VI are the following: The 23 Tank-rats, as a group, showed on the 11th day of training a distinct preference for the Dim route; and this preference became steadily accentuated from that day onwards. On the 20th day the Dim route was taken more than twice as many times as the Bright route; and, on the 30th day, it was taken five times as often as the Bright route. Contrasting strongly with this is the performance of the control rats. They, as a group, show no distinct predominance of (or preference for) the Dim route until the 30th day; and then the predominance is so slight as to indicate that only two or three of the rats (out of the 39) had mastered their task, *i.e.* had learnt regularly to avoid the Bright route.

The results embodied in Table VI are those on which I chiefly rely in claiming that my experiment has produced evidence supporting the Lamarckian hypothesis. After much reflection, I cannot see that they can be explained in any other way. They are set down here, together with the essential details of procedure, in order that they may be critically examined by those biologists who will refuse to accept them as such evidence.

In order to facilitate such criticism I present below a further table of figures, Table VII, showing in more detail the effects of the course of training on the 23 Tank-rats and the 39 control rats dealt with in Table VI. Here it is necessary to mention a feature of the procedure which was undoubtedly an imperfection, but not, I think, one which invalidates the results. In training (though not in testing) the rats, I dealt with them in batches, each batch being of rats of the same age drawn from one, two, or three litters, and consisting of from two to six animals. It would undoubtedly have been more satisfactory to have dealt with each rat and to have recorded his performances in the course of training separately and individually. But that would have involved the expenditure of much more time and labour on the same number of rats. A professor in active teaching work has not the time for research of his more fortunate colleagues in purely research-institutions; and I was forced to choose between dealing individually with a small number of animals and dealing in a less perfect manner with a considerably larger number. I chose the latter course, judging that the large number dealt with more than compensated for the imperfection of the procedure by batches. I might have obtained the assistance of graduate students, several of whom worked very faithfully for long periods on the other half-stock B. But, in view of the importance of the question at issue, the many possibilities of subtle errors, and the desirability, in case of a positive result, of being able to meet all criticisms with first-hand knowledge of every detail of the procedure followed, I had decided to keep in my own hands every detail of the handling of half-stock T. And this rule was followed throughout, with the single exception that the rats of this half-stock were sometimes fed by my assistants.

In this connection it is necessary to record the fact that, in training the batches of Tank-rats and of the controls, I frequently allowed as many as three rats to be in the water of the tank at the same time. Long experience has shown me that one rat is very little, if at all, influenced by the example of his fellows; and even if some slight influence of this sort was exerted, or even if it was much greater than I supposed, the fact would not invalidate the results; for, in this respect, the handling

TABLE VII.

Tank-rats					Controls								
3 T's	6 T's	6 T's	5 T's	3 T's	Day	5 B's	6 B's	6 B's	5 B's	3 W's	6 W's	2 W's	6 W's
9–9	19–17	15–21	10–20	9–9	1st	15–15	16–20	12–24	17–13	8–10	9–27	5–7	17–19
10–8	20–16	15–21	12–18	9–9	2nd	8–22	19–17	16–20	14–16	8–10	12–24	6–6	19–17
11–7	19–17	21–15	15–15	8–10	3rd	18–12	17–19	18–18	12–18	11–7	14–22	5–7	17–19
9–9	21–15	16–20	13–17	9–9	4th	12–18	15–21	19–17	11–19	10–8	16–20	4–8	22–14
10–8	17–19	18–18	16–14	10–8	5th	16–14	17–19	18–18	17–13	6–12	19–17	6–6	20–16
9–9	17–19	16–20	17–13	9–9	6th	14–16	19–17	16–20	14–16	10–8	14–22	4–8	11–25
7–11	20–16	20–16	12–18	10–8	7th	17–13	17–19	17–19	15–15	11–7	19–17	6–6	17–19
8–10	18–18	16–20	14–16	12–6	8th	16–14	18–18	19–17	14–16	9–9	18–18	7–5	18–18
7–11	18–18	19–17	12–18	11–7	9th	13–17	17–19	19–17	16–14	5–13	18–18	5–7	18–18
9–9	20–16	17–19	11–19	10–8	10th	15–15	15–21	17–19	16–14	11–7	17–19	6–6	19–17
12–6	18–18	16–20	14–16	12–6	11th	14–16	17–19	14–22	14–16	8–10	18–18	5–7	18–18
15–3	18–18	17–19	15–15	14–4	12th	13–17	17–19	16–20	17–13	9–9	17–19	4–8	17–19
16–2	20–16	17–19	16–14	11–7	13th	15–15	18–18	15–21	14–16	8–10	17–19	8–4	18–18
14–4	17–19	20–16	16–14	14–4	14th	15–15	18–18	18–18	15–15	9–9	16–20	10–2	16–20
12–6	19–17	20–16	14–16	13–5	15th	15–15	17–19	16–20	14–16	9–9	18–18	3–9	19–17
15–3	21–15	16–20	11–19	12–6	16th	13–17	17–19	16–20	15–15	9–9	18–18	4–8	17–19
17–1	18–18	21–15	12–18	12–6	17th	14–16	18–18	18–18	13–17	9–9	18–18	5–7	13–23
18–0	24–12	21–15	14–16	12–6	18th	16–14	18–18	17–19	16–14	8–10	17–19	7–5	13–23
16–2	24–12	24–12	13–17	11–7	19th	16–14	16–20	15–21	14–16	9–9	17–19	5–7	15–21
16–2	25–11	24–12	19–11	13–5	20th	17–13	18–18	19–17	15–15	9–9	20–16	7–5	13–23
17–1	24–12	25–11	15–15	17–1	21st	19–11	17–19	17–19	15–15	9–9	19–17	7–5	20–16
17–1	23–13	27–9	17–13	13–5	22nd	14–16	16–20	20–16	15–15	9–9	18–18	7–5	18–18
17–1	26–10	27–9	14–16	17–1	23rd	15–15	18–18	13–23	14–16	9–9	18–18	5–7	16–20
18–0	27–9	32–4	14–16	16–2	24th	21–9	20–16	18–18	16–14	9–9	18–18	6–6	14–22
17–1	26–10	30–6	14–16	18–0	25th	19–11	19–17	18–18	15–15	9–9	17–19	7–5	14–22
18–0	29–7	28–8	19–11	17–1	26th	19–11	17–19	17–19	18–12	9–9	18–18	8–4	16–20
18–0	29–7	29–7	17–13	18–0	27th	21–9	18–18	18–18	19–11	8–10	18–18	3–9	19–17
17–1	33–3	28–8	21–9	17–1	28th	21–9	16–20	18–18	15–15	9–9	18–18	4–8	16–20
18–0	29–7	32–4	22–8	18–0	29th	23–7	15–21	18–18	16–14	8–10	18–18	6–6	17–19
18–0	30–6	29–7	22–8	17–1	30th	22–8	17–19	16–20	18–12	9–9	19–17	7–5	18–18
—	—	—	—	—	31st	—	20–16	20–16	—	9–9	18–18	6–6	17–19
—	—	—	—	—	32nd	—	20–16	18–18	—	9–9	18–18	7–5	19–17
—	—	—	—	—	33rd	—	17–19	17–19	—	9–9	18–18	8–4	18–18
—	—	—	—	—	34th	—	18–18	18–18	—	9–9	19–17	6–6	21–15
—	—	—	—	—	35th	—	18–18	20–16	—	9–9	18–18	5–7	18–18
—	—	—	—	—	36th	—	20–16	18–18	—	9–9	20–16	5–7	18–18
—	—	—	—	—	37th	—	20–16	20–16	—	8–10	19–17	7–5	17–19
—	—	—	—	—	38th	—	20–16	20–16	—	9–9	17–19	6–6	18–18
—	—	—	—	—	39th	—	18–18	—	—	10–8	19–17	7–5	18–18
—	—	—	—	—	40th	—	19–17	—	—	9–9	18–18	4–8	18–18
—	—	—	—	—	41st	—	21–15	—	—	8–10	21–15	5–7	18–18
—	—	—	—	—	42nd	—	18–18	—	—	9–9	18–18	7–5	18–18
—	—	—	—	—	43rd	—	19–17	—	—	9–9	18–18	6–6	24–12
—	—	—	—	—	44th	—	—	—	—	13–5	16–20	10–2	24–12
—	—	—	—	—	45th	—	—	—	—	12–6	18–18	8–4	28–28
—	—	—	—	—	46th	—	—	—	—	15–3	18–18	6–6	—
—	—	—	—	—	47th	—	—	—	—	14–4	18–18	7–5	—
—	—	—	—	—	48th	—	—	—	—	15–3	18–18	5–7	—
—	—	—	—	—	49th	—	—	—	—	16–2	18–18	5–7	—
—	—	—	—	—	50th	—	—	—	—	18–0	24–12	6–6	—
—	—	—	—	—	51st	—	—	—	—	—	24–12	5–7	—
—	—	—	—	—	52nd	—	—	—	—	—	28–8	7–5	—
—	—	—	—	—	53rd	—	—	—	—	—	—	6–6	—
—	—	—	—	—	54th	—	—	—	—	—	—	9–3	—
—	—	—	—	—	55th	—	—	—	—	—	—	9–3	—

of the controls was entirely similar to that of the Tank-rats. The 23 Tank-rats were drawn from 7 litters; the 39 control rats from 11 litters (the 22 of half-stock B from 7 litters and the 17 rats of the new stock from 4 litters). The numbers of the sexes in each group (Tank and control) were approximately equal.

The following features of Table VII deserve attention. When compared batch by batch, there is only the slightest overlap between the records of the Tank-rats and of the control rats. The record of the best batch of control rats is very slightly better than the record of the worst batch of Tank-rats (bad and good, or worst and best, being here used to imply positions in the scale of rapidity of learning to master the prescribed task, *i.e.* to avoid the Bright gangway).

Of the Tank-rats, two batches, Nos. 1 and 5, showed marked preference for the Dim route on the 11th day of training; whereas no batch of controls showed any such preference until the 24th day, when it was shown by batch No. 1.

The worst batch of Tank-rats (No. 4) showed marked preference for the Dim route on the 26th day; in this respect it was slightly excelled by the best batch of controls (No. 1). This is the very slight overlap of the two groups.

It can be confidently inferred from the Table that, already on the 11th day, 2 Tank-rats and, on the 18th day, 8 of the Tank-rats had completely mastered their task (complete avoidance of the Bright route); whereas there is clear evidence that no one of the controls achieved this before the 24th day, when 2 of them appear to have done so.

There is clear evidence that on the 30th day not more than 7 of the 23 Tank-rats had failed to master their task; whereas, on the same day, of the 39 controls, 35 still failed, *i.e.* showed no indication of having learnt to avoid the Bright route; or, putting the same facts differently, by the 30th day, 16 out of 23 Tank-rats, but only 4 out of 39 controls, had learnt to avoid the Bright route; or, in percentages, on the 30th day, 69 per cent. of the Tank-rats, and only 10 per cent. of the controls had mastered the task.

It is further noteworthy that, whereas only 3 of the 23 Tank-rats had failed to master the task on or before the 30th day of training, 35 of the 39 controls showed no avoidance of the Bright route until later dates. Not all the batches of controls were trained as long as up to the 40th day; but, of those whose training was so continued, 23 still achieved no avoidance of the Bright route. The training of 11 control rats was continued beyond the 50th day, and, of these 11 rats, 9 achieved no avoidance of the Bright route until the 50th day or later.

Training-results of Preceding Generations.

It would have added greatly to the evidential quality of the results if it had appeared that the successive generations of Tank-rats had shown a regularly increasing superiority over the control rats, generation by generation. I cannot claim to have shown any such regular increase of facility in learning the task. This may be due in large part to the fact that, owing to lack of time, I did not make complete records of the course of training of all generations; and also to the fact that the procedure was changed (as described on p. 281) at the 9th generation, making the earlier and later records incomparable. But the failure to demonstrate any steadily progressive increase of facility in learning in successive generations is due also in part, I believe, to a fact which appeared very clearly in the results of the training of the half-stock B, and much less clearly in the records of the Tank-rats; the fact, namely, that there are seasonal variations in the general efficiency of rats, the whole of the half-stock showing seasonal rise and fall of efficiency in learning. Undoubtedly, what we vaguely call "intelligence" is a main factor in the process of learning; and vigour also is a factor, especially in the achievement of the task prescribed for the half-stock B, to be later described. It may be that these two factors, "intelligence" and vigour are really one, or that they are closely correlated. However that may be, this seasonal or periodic fluctuation of general efficiency throughout the successive generations renders the comparison of such generations with one another, or of any generation with control animals trained otherwise than strictly parallel in time, somewhat unsatisfactory; and further, if there be actual Lamarckian transmission and accumulation of the effects of training in the shape of innately increased facility of learning or acquisition, this periodic fluctuation of general efficiency must obscure the revelation of any such steady accumulation.

Nevertheless, it seems worth while to present in amalgamated form in Table VIII the results obtained with the 10th, 11th, and 12th generations of Tank-rats.

In Table VIII I have placed in the last column the figures showing the course of training of 7 rats of the half-stock B which were used as controls and trained parallel in time with the 16 Tank-rats of the 12th generation. I regret that I did not carry through the training process with a large number of controls parallel to generations 10, 11. and 12; only these 7 controls were trained, though a large number were put through the testing process. At this time I was relying chiefly on the testing process and had not realized that the more laborious training process was likely to yield more decisive results.

It will be noticed that these 7 controls made a rather better record than any of the seven batches of controls represented in Table VII; and, if they were amalgamated with the record of controls in Table VI, they would slightly improve that record. But more important is the exceptionally good record of the 9 Tank-rats of the 11th generation. It is distinctly superior to that of the best batch of Tank-rats of the 13th generation (cf. Table VII, batch 1). Already on the 4th day the preference for the Dim route is very marked; and by the 11th day all but two of the rats had mastered their task. I am at a loss to account for the marked superiority of the rats of this 11th generation. The 9 rats were drawn from 5 separate litters (2 = 2 = 2 = 2 = 1) and it is, therefore, very improbable, though possible, that 7 rats of this batch were all of strains of superior "general intelligence." This explanation is rendered the more improbable by the facts that their parents (the 8 rats of the 10th generation) showed no marked superiority, and that their offspring (the 16 rats of the 12th generation) made a record which, on the average, was only very slightly superior to the average or amalgamated record of the 13th generation.

TABLE VIII.

Course of Training of 10*th*, 11*th*, *and* 12*th Generations of Tank-rats.*

Day of Training	8 rats of 10th generation		9 rats of 11th generation		16 rats of 12th generation		7 control rats	
	Dim	Bright	Dim	Bright	Dim	Bright	Dim	Bright
1st	24	24	29	25	46	50	20	22
2nd	24	24	29	25	49	47	16	26
3rd	23	25	22	32	48	48	20	22
4th	24	24	35	19	46	50	20	22
5th	26	22	33	21	48	48	24	18
6th	21	27	35	19	50	46	24	18
7th	24	24	36	18	56	40	20	22
8th	25	23	42	12	50	46	22	20
9th	26	22	39	15	57	39	22	20
10th	22	26	47	7	57	39	18	24
11th	26	22	49	5	61	35	17	25
12th	29	19	47	7	58	38	21	21
13th	28	20	49	5	58	38	20	22
14th	24	24	—	—	66	30	19	23
15th	25	23	—	—	63	33	19	23
16th	28	20	—	—	71	25	20	22
17th	36	12	—	—	77	19	23	19
18th	37	11	—	—	81	15	22	20
19th	37	11	—	—	81	15	26	16
20th	39	9	—	—	80	16	29	13

The fact that the record of the 13th generation showed no advance upon the records of the 11th and 12th generations, but rather a falling off, justifies to some extent the assumption that the 13th generation represents one of those periodic declines of general efficiency which appear very markedly in the record of the 17 generations of the half-stock B. There is, therefore, some ground to anticipate that the following generations, the 14th and 15th, will show a marked superiority to the 13th, on which I have chiefly relied in making comparison with a large group of controls.

This periodic waxing and waning of efficiency in successive generations is a most disturbing complication; and it is, so far as I can see, the only factor which, just possibly, may invalidate the results obtained hitherto with the half-stock T as evidence in support of the Lamarckian hypothesis. I, therefore, add some further discussion of it at this point, before presenting the results obtained with the half-stock B, in which this periodic variation appears very clearly.

The breeding was arranged for both half-stocks so as to obtain young rats ready for training in February, May and October of each year; and the work of training was chiefly carried on in those three months, although usually extended into the following months. Throughout the period from February 1921 to February 1925, the performance of the half-stock B showed regular periodic variation of the following kind. The performance of the successive generations being plotted on a curve in which superiority is indicated by a low point and inferiority by a high point, the curve is one which regularly rises to a high point in February, sinks to a much lower point in June, and rises to an intermediate height in October. That is to say, the amalgamated records of the successive generations showed that the June generations did best, the February generations worst, and the October generations made an intermediate record. Now the task imposed upon the half-stock B was such that superiority in it was decidedly favoured by general vigour of the animals. I was therefore disposed to assume that in the spring, which is presumably the most natural breeding period, the rats are most vigorous, and that their vitality then sinks and reaches its lowest point about February. I am disposed still to regard this explanation as valid, although doubt is thrown upon it by the fact that the May records of 1925 and 1926 were unexpectedly high, that is to say poor. But both these departures from the rule are perhaps to be explained away. In 1925 our spring breeding had been delayed somewhat, so that the work of training fell chiefly into June, and the weather of that month was extremely hot. The animal-room being immediately under the roof, it proved impossible to keep its temperature reasonably low—it frequently approached 90° F. Now, as was mentioned above, the rats cannot stand a temperature of 90° F.; and it is reasonable to suppose that they suffered in this month some general depression of vigour through the unduly high temperature of the room.

This explanation does not apply to the poor record of the generation trained in May and June 1926. But in this case, most regrettably, one of my assistants, Miss Brackett, who had trained the 5 preceding generations, succumbed suddenly to an attack of appendicitis, after beginning the training of the 17th generation in May. The work was then completed by Mr E. C. Heck, to whom I am most grateful for his timely aid. He had taken a chief part in training the earlier generations in 1921 and 1922, and there is no reason to suppose that his handling was less expert than that of Miss Brackett. Nevertheless, a change of the personality handling the rats, occurring during the course of training, may well have had a subtle adverse influence sufficient to account for the inferiority of the performance of this 17th generation.

If it is proper, and I think it is, to distinguish "general intelligence" and general vigour as distinct factors, it may be said with confidence that vigour, while it contributes importantly to excellence in the task of the half-stock B, is not of the first importance in the task of the Tank-rats; and that, in their work, "intelligence" ranks as of the first importance. A high degree of vigour may even have been prejudicial to success in this task. It was noticeable that some exceptionally active and vigorous rats would continue to dash boldly across the electrified gangway after receiving many shocks from it; while less active or less bold and vigorous rats would, at a similar stage of training, hang about cautiously, hesitating before the Bright gangway, and perhaps turning away from it several times, before venturing to run across it. Such caution or timidity seemed to be favourable to their making the critical discovery of the constant correlation of brightness with the shocking quality of the gangway. In this way we may perhaps explain the inferiority of the 13th generation of Tank-rats (trained in May and June) to the two preceding generations trained in October and February.

If we provisionally regard the records of the Tank-rats as evidence of Lamarckian transmission of the effects of training, the question arises—What is the nature of that which is transmitted? Or better—What is that function which seems to be accentuated in successive generations of the Tank-rats as a consequence of the training process?

Three possibilities suggest themselves. First, it may be some increase of "general intelligence."

Secondly, it may be a general increase of timidity or caution, a greater susceptibility to fear, an increased strength or sensitivity of the fear-instinct.

Thirdly, it may be a specific tendency to avoid (presumably to experience fear at sight of) the brightly illuminated route or gangway.

It may be that two, or all three, of these modifications are combined. The first, an increase of "general intelligence" seems improbable. If this were the main factor, we should expect both half-stocks, B and T, to show it equally. But we find that the Tank-rats of the later generations showed (by comparison with the rats of the half-stock B which were used as controls) a marked superiority in respect to the task to which their ancestors of half-stock T had been trained. It is true that the controls taken from the half-stock B showed, in respect of this task, some superiority to the control rats of the newly obtained stock the ancestors of which had not been trained at all. This may be an indication that some general increase of "intelligence," or adaptability to new tasks, had been induced; but it is too slight to be taken very seriously.

The second possibility has more to be said in its favour. It was noticeable during the training of the later generations that in the main the Tank-rats showed a greater caution or timidity than the control rats. There were three pretty clearly marked stages in the training of each rat. First, a stage in which the rat, having received the shock on the Bright gangway several times, would, on approaching it, gather himself together, rush at it, and dash across it with great energy. Second, a stage during which he hung back in the neighbourhood of *O*, seemingly reluctant to take the risk of attempting either route. In some cases this stage became very troublesome and time-wasting, by reason of the rats' long delay before the increasing urge of the impulse to escape from the water overcame this reluctance, caution or timidity. Thirdly, a final stage in which the rat would either proceed always confidently to take the Dim route and walk quietly up the Dim gangway, or, if he entered the Bright route, would on catching sight of the Bright gangway, turn calmly about at once and proceed to emerge by the Dim gangway. Now, the Tank-rats of the later generations displayed the stage of cautious reluctance or timid uncertainty and hesitation earlier in the course of training and much more markedly than the control rats. This difference I noted with many successive batches.

Still the question remains—Was this increased timidity, this increased susceptibility to fear, a general one, or was it specific to the situation and brought into play only by the general situation, by the experience of receiving the shock or of seeing the Bright gangway? My data do not show any light on this question. An answer to it could be obtained only by a very prolonged and delicate investigation. Up to the present time I have thought it wise to concentrate wholly upon the fundamental problem—Is any effect of training transmitted down the generations? *I submit that the records of the testing and training of the Tank-rats do seem to constitute evidence of such transmission, and that, unless this appearance of Lamarckian transmission can be explained away in some fashion which hitherto has not occurred to me, they must be accepted as such evidence.*

The Training of Half-Stock B.

The task prescribed to the half-stock B and the procedure of training the rats to the task were fixed after some tentative experiments with the 1st generation, and have been applied to the following generations up to and including the 17th in the summer of 1926.

Our first notion was to train the rats to escape from some simple maze, using the usual incentive of food-reward. After some experimenting we found two very serious objections to any such plan. First, it is impossible to secure in the rats at the moment of training or testing a standard degree of hunger. And a little experience of such experimenting suffices to show that the rat (like man) works at his task more efficiently, seems to display more intelligence, the keener his hunger (within limits possibly), the stronger the urge within him to attain a goal. This question of incentives is all-important when it is desired to compare degrees of adaptability of an animal at one time and another, or of different animals.

Secondly, in the ordinary dry maze, the rats are apt in the early stages to loiter in a fashion most tedious for the observer. For, no matter how hungry the rat may be, he has to learn that food awaits him on escape from the maze, before the food-incentive can become operative; and the time occupied in making this discovery is almost entirely a matter of chance.

We therefore turned to the expedient of inducing the rat to escape from water; and we found that, though all white rats, from the age of three weeks onwards, can swim and dive in a most effective way, any rat when immersed in water makes strenuous and sustained efforts to escape from it. We therefore decided to use this urge to escape from the water as the fundamental motive in this experiment. It may be said at once that the urge to escape seems to be less strong in warm water than in cold. Immersion in water at or below 60° F. soon depresses the vitality of the rat. Therefore, we aimed always, though not with complete success, at keeping the water during these experiments as nearly as possible at 66° F.

The next step was to combine the maze-procedure with the water-incentive, *i.e.* to construct in a tank of water a simple maze such that, in order to escape from the water, the rat must run the maze[1].

We had to meet here the initial difficulty of teaching the rat that, by running the maze, he may escape from the water, in order that he may, throughout the training, have the incentive of a known way of escape, may have before him a definite goal towards which to direct his efforts. Of course, to a strict mechanist or 'behaviourist' what is here said of goal and incentive and motive must seem arrant nonsense. For their principles blind them to the importance of such considerations and indeed deprive these terms of all meaning for them.

Although our maze was simple, much experience makes it clear that not one rat in a hundred would ever succeed in finding his way out of it unaided, if confronted with the task of running the complete maze prior to all training. We, therefore, trained each rat by easy stages, according to a standard schedule to which we rigidly adhered. The maze consisted of a tank of galvanized iron similar to that used in the Tank-experiment, but not quite so long, *i.e.* about 3 feet by 20 inches and 20 inches in depth (Fig. 2). This contained water to the depth of 8 inches. It was divided by vertical walls of sheet zinc in the way indicated in the figure. In the zinc sheet holes were cut in the positions *A*, *B*, *C*, *D*, *E*, at the water level, through which holes the rats could freely swim. At *P* is a wooden platform projecting half an inch above the surface of the water. A rat immersed in the water of this bath (we spoke of it always as the bath and it is convenient to speak of the Bath-procedure in distinction from the Tank-procedure) could escape from the water only by finding his way to *P*.

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[1] I believe that the first suggestion of this combination came from Mr Chi Li, who co-operated in the early stage of the experiment. As the water-maze promises to be of great value in animal psychology, Mr Li, who is now, I believe, professor of psychology at Tientsin, should have the credit of this suggestion. I was much interested at the recent Congress of Psychologists at Groningen to find that Prof. Gopalaswami of Mysore has independently begun a somewhat similar experiment directed to the same end, namely, light upon the Lamarckian question. His method, which he demonstrated kinematographically, consists in training white rats to dive to the bottom of a tank of water in order to obtain food. The experiment is still in a quite early stage. I may say here that, since my procedures with both half-stocks involved training rats of a number of generations to dive, I was on the look-out for any indication of increasing readiness to dive or to take to the water. The few careful observations I have made, in the way of comparison in this respect between young rats of my trained stock and those of a control stock, have failed to yield any indication of a positive result.

On the 1st day the doorway *E* was closed, and the rat was placed three times in the water at the point marked 1 in the figure. He invariably escaped to *P* within a very short time. In this way, *P* became for him a goal which he sought, towards which he actively strove. On each succeeding day he was immersed in the bath six times. On the 2nd day he was placed in the water at the point 2, a vertical wall having first been slipped into position just beyond 2, as indicated by the dotted line. On the 3rd day he was put in six times at *O*, the barrier at 2 being removed, and the doorways *A*, *B*, *C* and *D* being closed by close-fitting plates. On the 4th day he was again placed six times at *O*, but on this day all the doorways, *A*, *B*, *C*, *D*, as well as *E*, were open, giving him the possibility of entering the four blind alleys. On the 5th day he was placed again six times at *O*; and, on this day, an additional difficulty was presented in the form of a vertical barrier indicated by the dotted line at *Y*. The lower edge of this barrier was, on the first occasion, sunk ½ inch below the surface of the water. On the next and all subsequent occasions its lower edge was sunk 3 inches below the surface. In order to escape, the rat had now to dive under this barrier *Y*. On the 6th day a second similar barrier was placed at *Z*; this also having its lower edge 3 inches below the surface of the water; and the rat was again immersed six times at *O*. On later days no further change was made in the maze; the rat was placed each day at *O* six times; and escaped six times by diving under *Y* and *Z* and swimming to *P*. And this was repeated every day (or nearly every day) until the rat was ready for mating and breeding about the middle of his fourth month after birth. This, at least, was the course of events with the majority of the rats.

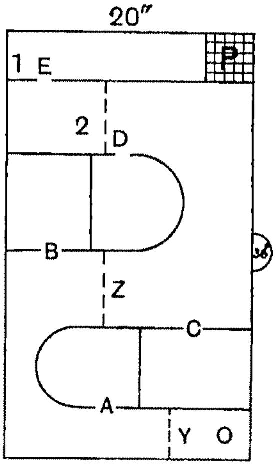

Fig. 2.

With some few rats the difficulty arose that the six immersions of one day did not suffice to enable the animal to master the new difficulties presented on that day. This occurred most frequently on the 5th day, when, for the first time, the rat found himself in the water at *O* enclosed in a small space from which he could escape only by diving under the barrier *Y*. It is perhaps surprising that so few rats failed to achieve this step at the first trial. The question arose—how to deal with those rats that failed to achieve a new step within the prescribed time. We found that it was useless to allow a rat to remain in the water longer than three minutes on any one occasion. After that period of immersion a rat shows signs of diminishing vigour; and soon afterwards begins to drown, if not removed from the water. We adhered, therefore, to the rule of removing from the water at the end of three minutes any rat that had not escaped by his own efforts within that period. And we followed the further rule that any such rat should, at the next two immersions, be put back to the preceding stage, and then allowed to try again under the more advanced, the more difficult, conditions. This procedure overcame this difficulty in a fairly satisfactory manner.

On each occasion of immersion of each rat during the first 12 days of his training, the time during which he remained in the water, *i.e.* the time from the moment he was placed in the water until the moment of his climbing out on *P*, or (in case of failure) the moment he was removed from the water, was measured with a stop-watch. And these time-records for each day's six performances of each rat were added together, giving a figure which expressed his total time in the water on each day of training.

In each generation we trained at least 9 rats, 3 males and 6 females. Of some generations we trained more; and we should have liked to train some 30 or more rats in each generation; but the procedure necessarily consumed much time. The time-records of all the rats of each generation were then averaged for each day of training (up to and including the 12th day) giving, for each day of training of that generation, a figure expressing in seconds the total average time spent in the water on the six immersions of each day, or the average performance for each day of training, for all the rats of the same generation, expressed in terms of total time in water on the six immersions of that day. Then on squared paper we plotted a curve representing the average performance of each generation during its first 12 days of training, in the following way.

The 12 successive abscissae from left to right represent the first 12 days of training. The ordinate of each day represents the total average time spent in the water up to and including that of the six immersions of that day. That is, the ordinate of the 2nd day represents the average time in the water on the three immersions of the 1st day plus the average time on the six immersions of the 2nd day. The ordinate for the 3rd day represents the ordinate of the 2nd day plus the average time spent in the water on the six immersions of the 3rd day and so on.

By drawing a line through the upper extremities of the ordinates we obtained for each generation a curve sloping up to its highest point at the 12th day. The total height reached on the 12th day is the most significant feature. The form of the curve represents the progress of training. The total height, which may also be expressed numerically, represents the total time spent in the water by the average rat of that generation throughout the 69 immersions of its first 12 days of training. Excellence of performance is, of course, inversely as the length of time spent in the water.

Our plan was, then, to compare the curves thus obtained for successive generations, and also to compare the curves thus obtained for each generation trained with similar curves obtained from batches of control rats. If the curves for successive generations should show a steady fall of height (*i.e.* if the total average time of each generation should be less than that of the preceding generation) we should have strong evidence of Lamarckian transmission of the effects of training. If, after some generations of such training, the animals of the trained half-stock should show in respect of this task no trace of superiority over the early generations of their ancestors and no superiority over animals of control stock untrained to this task, this result would show that no Lamarckain transmission had taken place and would be evidence against the Lamarckian hypothesis.

In handling this half-stock B and in mating and selecting animals for breeding and training, the same precautions were taken as with half-stock T; *i.e.* especially we provided against a favourable selection of animals by effecting selection at random from the young litters; merely aiming at a due proportion of males and females, *i.e.* two females to each male.

The general results of this procedure obtained with the first 17 generations are presented numerically in Table IX.

In Table IX, the first column gives the number of the generation of B rats; the second, the date of the training of that generation; the third, the average total number of seconds spent in the water by the rats of each generation during the first 12 days of training; the fourth, the same average time of the best or quickest 3 rats of each generation; the fifth, the average time of the rats of three successive generations, grouped in the manner indicated by the brackets; the sixth, the total time of the best or quickest rat of each generation.

It is to be remarked that the figures for the first generation are perhaps unduly high, owing to the fact that at this time we had not become expert in the handling of the rats. The time taken by the 17th generation is also anomalous, namely, unexpectedly high. I have already mentioned that this may well be due to the unfortunate sickness and death of one of my assistants which occurred in the midst of the training of this generation. Unfortunately, the figures for the 9th and 13th generations were not completely recorded and therefore cannot be brought into the table for comparison. The tendency for the May records to excel those of February and October appears well marked in the earlier years.

TABLE IX.

Results of Bath-procedure with 17 *Generations of Half-Stock B.*

Generation 1	Date 2	Average time of all B rats 3	Average time of best 3 B rats 4	5	Total time of best rat in each generation 6
1st	Feb. 1921	2320	1886		2060
2nd	May ,,	1340	921	1002	800
3rd	Oct. ,,	1540	969		752
4th	Feb. 1922	1950	1117		1020
5th	May ,,	1180	793	1010	620
6th	Oct. ,,	1610	1081		1020
7th	Feb. 1923	2240	1258	975	1080
8th	May ,,	980	810		732
9th	Oct. ,,	—	—		—
10th	Feb. 1924	1980	859		806
11th	May ,,	1250	969	846	890
12th	Oct. ,,	1140	725		681
13th	Feb. 1925	—	745		703
14th	May ,,	1480	1034	787	980
15th	Oct. ,,	1020	743		710
16th	Feb. 1926	1020	584		495
17th	May ,,	1620	1111	—	1009

The feature of chief interest presented by Table IX is the general improvement or shortening of the times with the progress from the earlier to the later generations. This appears in all the columns, 3, 4, 5, and 6; most smoothly in column 5, and most markedly in column 6, where we see that in the 16th generation one rat proved greatly superior to all others.

It is, I think, legitimate to lay stress on the comparison of performances of the best three rats and of the best individual rats of each generation, for two reasons: first, there are large individual differences of "intelligence" or adaptability between the rats; and, if any batch of rats happens to include one or two particularly stupid or slow individuals, the very long times of these few rats (or of a single such rat) unduly weights the average of the batch; for the time spent in the water may be, and sometimes was, very much prolonged beyond the average time; whereas the best or quickest individuals cannot excel the average time in the same degree, they can only approximate to a certain minimum time which is of unknown duration, but probably lies in the neighbourhood of 400 seconds. Secondly, there is some slight indication that, among the descendants of trained rats, some individuals show well-marked evidence of inheritance of the effects of ancestral training, while other individuals show us no such evidence; that is to say, there is some indication that the transmission of effects of training follows a law of segregation reminiscent of Mendel's law, the effect being transmitted to some individuals and not at all to others. This indication consists in the fact that the rats of the trained stock show a much wider range of variation than the control rats of untrained stock, *i.e.* while the slower individuals of the trained stock are as slow as any of the untrained stock, the better or quicker individuals of the trained stock are very much quicker than the quickest of the untrained stock. Thus only 2 rats of the untrained stock (out of a total of 29 control rats) made records of less than 1000 seconds; while of the trained stock (out of a total of 153 rats) no less than 28 individuals made records less than 1000 seconds.

Since the periodic variation of general efficiency from generation to generation occurs in the troublesome way discussed on a previous page, making the comparison of successive generations of the trained B stock unsatisfactory, it seems that we should chiefly rely here, as in the case of the T stock, upon comparison with control rats. We had not realized this necessity in the earlier stages of the experiment, and had proposed to rely chiefly on the comparison of successive generations. We had, therefore, used no controls during the first 4 generations. But, during the training of the 5th, 7th and 17th generations, we put controls through the training process, strictly parallel in time and under conditions exactly similar to those obtaining for the rats of the B stock, using in all 29 control rats (8 parallel with the 5th generation, 11 with the 7th, and 10 with the 17th). Most of these control rats were of the half-stock T; but 6 of those trained with the 17th generation were of the stock newly obtained from the Wistar Institute. Their performance did not differ essentially from that of the T rats.

Comparing the average total time of the 29 control rats (1622 seconds) with the average total times of the 17 generations of B rats (Table IX) we see that 11 of the latter gave lower average time than the controls, and only 6 of them gave higher average times.

We may compare also the average times of the best three rats in each of the 17 generations of B rats with the average time of the best three control rats, out of the total number of 29 controls, namely 973 seconds. We then see that 10 of these averages surpass this record of 973 seconds, some of them being very much superior.

TABLE X. *Results of Training Control Rats in Bath-procedure.*

	Average total time	Average total time of best 3 rats
8 (T rats) trained with 5th generation B rats	2040 secs.	1342 secs.
11 (T rats) ,, ,, 7th ,, ,,	1267 ,,	973 ,,
10 (4 T rats, 6 others) ,, ,, 17th ,, ,,	1679 ,,	1320 ,,
Total of 29 control rats gave average total time =	1622 ,,	

Further, we may compare the best individual rats in the 17 generations with the best individual among all the 29 controls. The record of the latter was 934 seconds. Among the individual records of the B rats that I have by me are records of 25 B rats which surpass this figure, 934 seconds; and I have no doubt that by searching thoroughly the original records more could be found.

Finally, and these, I venture to think, are the most significant figures, if we take the average of the records of the 10 best control rats we find 1160 seconds; while the average of the best 10 B rats (all of which are found in generations later than the 4th and of which 2 only occur in generations earlier than the 12th) is 669 seconds.

It may be objected to this last comparison that the best 10 control rats are selected from among only 29 animals, whereas the best 10 B rats are selected from a number about five times as large. Therefore, in order to avoid this objection, I select other figures. Let us compare the best 3 rats out of the 29 controls and compare them with the best 3 from the first 3 generations of B rats and the best 3 from the last 3 generations of B rats. In this way we obtain groups of almost equal numbers, the advantage of numbers being slightly on the side of the controls. Against the figure 973 seconds (representing the average time of the best 3 controls), we have then 825 seconds (average time of best 3 B rats of first 3 generations) and 585 seconds (average time of best 3 B rats of last 3 generations).

Or again, the best 10 rats of the 29 controls (with the average record of 1160 seconds) may be compared with the best 10 out of the similar number of the first 3 generations of B rats (giving the average record 1237 seconds) and with the best 10 out of the similar number of the last 3 generations (giving the average record of 848 seconds).

And, if we put aside the anomalous 17th generation, and take the best 10 rats from the 14th, 15th and 16th generations, as we fairly may do, we get a figure, namely 785 seconds, still more favourable to the B rats of the later generations. This figure contrasts strongly with that of the best 10 controls, namely 1160 seconds.

Summing up, it may be said that, however the records be compared, the B rats of the later generations showed a very marked superiority in respect of facility in learning to run the maze (a facility measured in terms of total time spent in the water) both over the B rats of the earlier generations and over the control rats. And, when we compare the better or more efficient members of these three groups, the superiority of the rats whose ancestors have been trained to the task for a number of generations comes out more strongly.

Discussion of Results of both Procedures.

Both procedures have, then, hitherto given results which seem to support the Lamarckian hypothesis. I regard the results of the Tank-procedure as affording stronger evidence than those of the Bath-procedure, for the following reasons: first, the superiority of the rats of the trained stock to the control rats is more clearly marked, although the number of generations trained in the Tank-procedure was smaller.

Secondly, the Tank-procedure is, I think, superior to the other; it is better designed to bring out any Lamarckian transmission that may occur; and it induces in the animals a kind of change which, as I judge on somewhat indefinite grounds, is more likely to be transmitted than the change induced by the Bath-procedure, the former producing a new conative set, the latter only a modification and greater definiteness and precision of the sequence of bodily movements through which a conative impulse works out to its goal.

Thirdly, the whole of the handling of the half-stock T was confined to my own hands; whereas I was compelled to leave the handling of the other half-stock almost entirely to several of my assistants; and therefore I cannot personally guarantee the impeccable conduct of every detail of the Bath-procedure throughout the six years of the experiment, as I feel able to do in the case of the Tank-procedure. I have every reason to believe that my assistants worked very faithfully and conscientiously; and I was constantly in close touch with them, maintaining a close personal supervision of their work. In this connection it is necessary to avow that, during the course of the experiment, there grew up in all of us a keen interest in, I think I must in fairness say, a strong desire for, positive results. From the first it was obvious that a positive result would be more striking, would excite more interest in the biological world, than a negative one. And, when indications of a positive result began to appear, it was but human nature to desire that this result should appear as clearly cut and positive as possible. Further, on my own part, there was a feeling that a clear-cut positive result would go far to render tenable a theory of organic evolution, while a negative result would leave us in the Cimmerian darkness in which Neo-Darwinism finds itself.

I was conscious, therefore, of a strong bias in favour of a positive result; and throughout I was consciously struggling against the temptation to condone or pass over any detail of procedure that might unduly favour a positive result. Such details are encountered at every point, more especially in the breeding of the animals. To have disguised from oneself this bias, to have pretended that we were superior to such human weakness, would have been dangerous in the extreme; the only safeguards against its influence were the frank avowal of it and unremitting watchfulness against it. I can conceive of no task that could make greater demands upon the scientific honesty of the worker; and it is in part this demand for unremitting watchfulness that renders the work peculiarly exhausting. I can only say that I believe we have succeeded in standing upright; and in fact, for myself, I am disposed to believe that I have leaned over backwards, as we say in America. Whether we have really succeeded in this, the most difficult part of our task, can only be proved when other workers shall have undertaken similar experiments. If our results are not valid, the flaw, which escapes our penetration hitherto, must, I think, be due to some subtle influence of this bias.

Some critics will point to the small number of animals trained in each generation. This is undoubtedly a weakness of our experiment. But it is one that could be avoided only by a research-worker, or group of workers, free to give his, or their, whole time to the work. I therefore conclude with the statement of the opinion I formed when still an undergraduate, namely, that there is urgent need for an ample endowment upon which a well-designed experiment of this kind might go forward for many years, with some guarantee of its continuance in spite of changes of personnel. For an answer to the question—Does Lamarckian transmission occur?—is imperatively needed by biology and by all the social sciences.

A last word upon the bearing on the social sciences. It is obvious that not only the theoretical basis, but also the practical prescriptions, of all the social sciences, politics, education, and all the rest, must be profoundly affected by a clear answer, positive or negative, to the Lamarckian problem. The bearing of such an answer on eugenic principles is perhaps the most obvious of all. I have long taken an ardent interest in eugenics, and have written upon its problems at some length: and in all that I have written I have assumed provisionally that the Neo-Darwinists' denial of Lamarckian transmission is substantially correct. This fact might well have engendered in me a strong bias against acceptance of evidence in favour of Lamarckism. But it has not done so, because I believe that, if the Lamarckian principle is valid, the social outlook, the prospect before our civilization, is very much brighter than if it be illusory. For if it be valid, eugenics can work harmoniously with euthenics. If it be not valid, the conflict between eugenists and euthenists would seem destined to continue while our western civilization declines and decays, it will then remain for some non-Christian people to carry on the torch of civilization.

But, if Lamarckian transmission, such as is indicated by our results, really occurs, the fact should not render us any the less ready to support well-considered measures of eugenic research and practice; for it seems clear that Lamarckian transmission, if it occurs, requires many generations to produce appreciable effects in the way of improvement of stock. And our civilized stocks require a vast amount of improvement before they can be contemplated with satisfaction. It is to be remembered that our 17 generations of trained rats would represent in human terms a period of some five centuries. And I cannot see any sufficient ground for hope that our western civilization may maintain itself through five centuries, unless some great improvement of the quality of our stock shall be effected in the course of those centuries.

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THE CREATIVE PROCESS IN THE ARTIST'S MIND[1]

BY S. ALEXANDER.

THE object of this paper is a limited one. I do not profess to give anything more than a tentative answer to the question in the title; my desire is to provoke discussion and inquiry.

I take as a text part of a passage in Mr C. E. Montague's recent novel *Rough Justice*. The hero of the story has brought with him to Oxford along with a naturally lively mind the tepid interest in intellectual things for which he has been prepared at a Public School, and is supposed to be reading for Greats, part of which is philosophy. "No doubt philosophy in bulk was stodge as every one said; still there seemed to be some fetching notions knocking about in it; that one about matter and form was great—the idea of 'form' as all the purposeful rightness and beauty there is in a thing or person—the lovely perfect statue that's somewhere in any block of stone, only waiting for the right man to chip away all the clogging formless matter that always rounds it and keeps it from being the topping thing that it might be. Fine too the notion of how you didn't really have a feeling first and then express it, while it just stayed where it was—how the feeling itself was changed, just by being expressed; you let a yell, and behold the feeling that made you yell had moved into some new state of itself—it had changed by issuing in that yell; and so expression *was* feeling in a way—a mode of feeling, one of feeling's ways of completing itself!" The latter is the thesis which in a more or less modified and perhaps exacter form I wish to present to you. In other words my topic is the connection between the artist's expression and the imagination and emotions from which it proceeds.

For convenience I will make some preliminary remarks, some of which border on philosophy, in order to pass on. First of all let us distinguish between creative imagination and passive imagination. The second we have not only in ordinary memory or expectation, but in one species of what is commonly called constructive imagination, in daydreams or reveries or in dreams in general, that is the comparatively idle play of fancy. I say comparatively idle, for there are no hard and fast lines of distinction in these matters of the mind's action; and dreams in particular are thought to be directed by a hidden purpose. The difference of such passive imagination from the creative sort is the absence of purpose. In the first, images flare up in your mind, more or less like the perceptions you have had, or in new combinations which have had and will have no real existence. In the creative imagination of art, on the other hand, you are not merely contemplating objects of the real world as they come to you faded or distorted in memory or expectation or heightened by more or less passionate fancy, but you create a new reality by the mind, lift objects selected from the real world into a new world in which they possess not mere existence, but something more which is value, the value of beauty. And you do this by moulding the material to express a purpose or if you like a thought. The purpose may be dictated by passion, but it is still a passionate *purpose.* There need be no mystery about the word purpose. For when our purposes are most conscious, we mean only that action is controlled by ideas which are other than those provoked by the immediate stimulus to action, and are supplied by the agent himself. Such controlling factors are present in the work of art, by no means necessarily, perhaps rarely, consciously purposive, most often present as a dominant passion, which guides the artist more surely than conscious ideas, but yet unifying his choice of words or colours or sounds into an expressive whole.

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[1] An address to the Aesthetic Section of the British Psychological Society in 1926.

Next, the act of the artist is expressive, and in two in particular of the many senses of that word. It is the issue into material form of the artist's mind, and I am going to speak of it mainly in this sense. It is expressive also as significant of a certain subject matter which the artist seeks to express (in the first sense); as when it is said that art embodies the characteristic or the essential meaning of its topic.

Thirdly, its difference from science is that it is not merely descriptive of something given which controls the description, but is subject to a double control, partly like science from the subject matter, but partly from the artist's own mind and store of thought and feeling; so that the work of art is never a faithful reproduction of its topic, whereas the art of the man of science is to secure fidelity to his original. Wordsworth was right when he said that the real distinction was not between poetry and prose, for of course prose may be art, but between art and science.

Finally, to close these desultory remarks, art is essentially communication. It is not practised in order to communicate to others thoughts or fancies which otherwise the poet must keep to himself, and still remain as much a poet; he is not a poet till he has spoken or a painter till he has painted, and produced something outside himself. But this statement is really an anticipation of what I am to try to expound.

I come to immediate grips with my subject when I next say that the creative action of art is as much an organic reaction as a sensory or perceptual response. Without describing or discussing the intermediate process I may say that the poem is wrung from me in the same way as a blow is wrung from me by a man that injures me, or as I turn my eyes to see a colour or sniff up a smell with my nostrils. I must be dogmatic only to be clear. When I am said to see a colour, what happens is that the rays of light acting on my retina compel me to turn my eyes and the colour is before them. A physical object produces an excitement in a highly complicated nervous structure, which acts in a certain way, and its action is our consciousness of the object. This may sound like philosophical invention; it is really psychological description. The persons who say that we first see the colour or have the sensation of colour and then turn our eyes are really the inventive geniuses. Now a poem is wrung from a poet in the same sense. His words are his reaction to a particular subject matter, say a woman he loves. Only there is a difference. Ordinary practical response to objects issues in alteration of the objects or alters our relation to them. I strike the man who hurts me and dodge the approaching motor-bicycle. My action terminates upon the object which provokes it. But the creative reactions of art instead of affecting the provoking object produce a new one, a statue or a poem or a song. That is why the reaction is a productive one and not merely practical.

Yet there is a gradual transition from the one class of response to the other. The simplest illustration is that of speech. First interjections and then ordered speech are physical expressions of our minds as the angry blow is the expression of hatred. There is little difference apparently between the blow and saying 'I hate you.' The beginnings and the possibility of art are found in these natural gestures. But only the possibilities and not the essence. It is a long way from saying passionately 'I love you and always shall' to saying

As fair art thou, my bonnie lass,
  So deep in luve am I;
And I will luve thee still, my dear,
  Till a' the seas gang dry.

The reason is that these words are not a mere half-practical gesture appropriate to the passion; but that the words which we speak in passion, being a new thing different from the object of the passion, can be handled for their own sakes, they are objects to be observed and manipulated. As mere joyous leapings and turnings may be controlled to express a purpose and a dance be created which is a work of art; so words which are in the first instance mere sounds wrung from passion, are a set of things which by selection and combination and variation and heightening and lowering and modulation are manipulated for their own sakes, so as altogether to be emancipated from practice. For words in the first instance are used as subservient to practice; they serve as directions to practical ends. They become art when they, with the meanings they contain, cease to serve private purposes but become public objects which are expressive of the artist's meaning.

There is a further observation to make about this transition, which is of importance for understanding the part played by feeling in the creative process. The blow of anger is the issue of a different part of the total mental state from the added words 'I hate you.' The first satisfies the bare passion which we can share with brutes; the second affords a different satisfaction, the satisfaction of speaking one's mind, and that satisfaction is something which includes the brute passion but supersedes it, and in a manner refines it. When we come to art and creativeness, this feature disengages itself further, and appears as the impulse to creation. That impulse is fed by the passions aroused by the subject of the art, but is not identical with their bare impulses, but in a manner independent of them, or rather is superadded to them as their fine flowering into something they themselves do not contain, as you may recognize at once by reverting to the stanza I quoted from Burns. Just so in science, besides the mere aspect of natural objects, which the common mind takes in equally, there is excited the impulse of curiosity, which attempts to unify these various impressions into an understanding whole. In conduct Aristotle has put the same point once for all, when in describing bravery he contrasts that virtue with the impulse to show pluck and brave danger, and says that in true courage there is the impulse towards the beautiful (τὸ καλόν). It is this which I suppose is what the Herbartians meant or were feeling after when they spoke of the aesthetic and other feelings of value as formal. It is as necessary to recognize their special status and the special impulses to which they belong as it is not to forget that except as including the impulses which feed them they cannot operate. You cannot be generous without having something to give, were it only the widow's mite; nor pursue science without data; nor hardly indite a ballad to a mistress' eyebrow without some basic tenderness.

I say 'hardly,' for the point lies here. Excellent sonnets, I am told, have been written which proceed from no actual passion, and some have supposed that even Shakespeare's sonnets were some of them mere exercises in gallantry after the Italian fashion of the time. Lyrical poetry in particular and perhaps music always (for I do not know enough to say) are apt to mislead us into thinking that the work of art is the expression of the material (as distinct from the formal) feeling which possesses the artist's mind. But dramatic poetry hardly allows this misunderstanding. We cannot suppose that Shakespeare wrote out of emotions in himself which resemble those which his characters display. Those emotions supply him with his material or a part of it; the emotion so far as he had it under which he wrote was proper to the artist getting himself inside his subject. Doubtless his own tastes and character may in part be guessed from his varying sympathy with his subjects. Yet his peculiar emotion was that which attended or was identical with his creative impulse, and accordingly we must assent I think to Mr Clive Bell[1] in this matter. The neglect of this consideration, though it stares you in the face in Burns's lyric, leads to some strange beliefs about the artist, which have even the authority of Wordsworth, who for all the wisdom and inspiration and beauty of the famous prefaces to *Lyrical Ballads* was a better poet than he was a critic of art, and, as Coleridge showed, did not practise what he preached even in his use of language. There is first the notion that there are many poets in the world who merely lack the gift of words, and next what follows from this that poetry is merely a superadded charm which enables the poet to give pleasure in the portrayal of the situations he describes. His own words about the poet give a truer view; "he murmurs by the running brooks a music sweeter than their own." This second point I will delay for the present till I speak of artistic expression. The first is contained in the lines of *The Excursion* which everyone knows:

Oh! many are the poets that are sown
By Nature; men endowed with highest gifts,
The vision and the faculty divine;
Yet wanting the accomplishment of verse.

Poets and all artists it will be admitted are more sensitive to things and persons than ordinary men. Such greater sensitiveness does not however make them poets. You have only to compare the magnificent lines describing the mystical absorption of the youth in the spectacle of nature, in the same poem[1], with Spinoza's scientific account of the "intellectual love of God"; in order to recognize that two great men may have like emotions and the one be a poet and the other a philosopher; and the expression of each be perfect in its kind, but that of the one a poet's work, and that of the other a philosopher and scientific man's. In the second case the words only catch fire from the subject matter; in the other the words are themselves on fire.

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[1] In his book *Art*. London.

What I am suggesting is that it is not the subject matter that makes the poem, but the words (including of course their meaning); though doubtless the greatest poems in the long run are occupied with the greatest subject matter, with themes of high import. And I am suggesting that it is a mistake to think, as many do, that the object of art is to provoke in the mind the emotions appropriate to the subject matter which was the artist's stimulus to production. Tolstoy's doctrine rests upon this misapprehension; and it is the source of that imperfect and sentimental appreciation of art which grows eloquent over the passions so vividly portrayed in a picture. The emotion which the work of art exists to communicate is the aesthetic emotion from which the artist created. Else we could hardly bear to look upon Michael Angelo's Last Judgment in the Sistine Chapel; or should turn away in disgust at the subject from Tintoretto's picture in San Rocco of that saint's ministrations in the hospital, though indeed I am not pretending that that picture belongs to the greatest art or even to the greatest art of its master.

I am now arrived at my immediate topic, and I begin with distinguishing between the subject matter of the artist and the product which is the work of art and is contained in words or colours or marble or sounds (when I say words or sounds I mean actual spoken words or actual musical sounds and not the mere notation of them in writing or print). My object is to indicate the part played by images in the process of creation, and accordingly it is vital to distinguish the images excited by the subject matter from the images of the artistic product itself. My thesis is that in artistic creation an artist starts with some subject matter: it may be a person whose portrait he is painting, or his own love which he is describing in verse, or Claudio's appeal to Isabella to sacrifice herself in order to save his life. That the subject matter throws him into an excitement which is distinguishable from, though dependent on, the emotions or thoughts which, owing to his peculiar gift of penetration, the subject matter arouses in him; and that the work of art issues from this excitement, the creative excitement, as any other action issues from the excitement which its stimulus provokes in the mind. Now the subject matter, which we may most conveniently suppose to be present in sense, calls up or may call up in proportion to the artist's wealth of thought or of experience, all manner of imagery. Such images are imaginations about the subject matter. They are not images of the artistic product; they are not images of what the artist means to say or to produce. In general (the qualifications must come later) there are no images of the product at all. The artist himself knows what he means to produce only when he has produced it. Just as, to revert to my dogmatic statement above, a person first knows the colour presented to him when through the physical action of the colour on his retina he turns his eyes to the colour and is said to see it. When I am on my bicycle on a dark night and a motor-car bears down on me with all its effrontery blazing from its eyes, I do not think I must get out of the way of this danger; I just get out of the way, and then think 'Well, I have escaped that danger,' even if being blinded I have nearly fallen into the ditch. True, I have had experience of the danger of meeting bodies in violent motion, and that experience is recorded in the impulse which directs me to turn aside. But the first time I experienced the danger it was the violent motion which frightened me into turning aside and thus revealed itself to me as dangerous.

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[1] Sound needed none,
Nor any voice of joy; his spirit drank
The spectacle; sensation, soul, and form,
All melted into him; they swallowed up
His animal being; in them did he live;
And by them did he live; they were his life.
In such access of mind, in such high hour,
Of visitation from the living God.
Thought was not; in enjoyment it expired.
No thanks he breathed, he proffered no request;
Rapt into still communion that transcends
The imperfect offices of prayer and praise.
His mind was a thanksgiving to the power
That made him; it was blessedness and love.

The artist is in a similar position. Only his mind is a much more complicated thing. Still it is his creative impulse which issues in the words he speaks or the movements of his brush or his chisel. The creative impulse is purposeful, and is controlled first by the present subject matter and next by the thoughts and images which gather round it. But these images and thoughts need not be present before him; they may make themselves felt merely as a vague tendency or attitude directing his creative movements and not otherwise emerging into life;

they may merely alter or refine or distort the emotions attendant on the subject matter, as my past experience of danger on the road affects the mere general impulse to swerve aside from terrifying objects. The artist does not merely create; he creates something which is in relation to the subject matter that stirs the creative impulse. Only in this sense does he express or represent the subject matter. But when he has achieved his product he knows from seeing or hearing it what the purpose of his artistic effort was. He makes the discovery of what were the real directive forces of his action. All that he was before aware of was the thoughts and emotions of the subject matter directly produced or indirectly suggested and doubtless often present in imaginative form. These combine with or in part are identical with the more or less unconscious movings and emotions yielded by his vision and divine faculty and with the gathered expertness of his technical *flair*, to guide his hand or his voice or speech into the movements which end with the work of art.

Accordingly my only difference from Mr Montague's text is that he simplifies rather too much for my purpose, though not for his own, and his graphic statement of the truth is a little wanting in precision. The yell which you let does alter your state of mind; it transfers your consciousness from the immediate provoking cause of the yell to the yell itself and makes you aware of the subtleties which gave the yell its peculiar colouring. The yell completes the artist's state of mind, and makes him realize (and with him the spectator or hearer of his work), that there has been substituted for the old subject matter something new which is the artistic product. That product is the expression not of the old subject matter and its emotions but of the whole mass of creative impulse and emotion (including and refining the original basic feelings belonging to the subject matter). It has to be remembered that the unifying impulse which issues immediately in the work of art is not on precisely the same level as the provoking impulses but organizes and changes them and directs them into a new channel. With this reservation I accept his illustration.

What is important in the doctrine is that it makes the external material work an organic part of the creative process. It ceases to be possible to say that the work of art is merely the rendering into material form of images which are the real work of art, still more to say with Wordsworth that what the poet adds to the man with the vision is "the accomplishment of verse." Recur once more to Spinoza, with whom the vision was probably more profound than with Wordsworth. Spinoza has his vision in thought which he puts into descriptive words, if the words are not themselves the thought. With Wordsworth the vision comes through the words; he is not thinking but feeling his human unity with nature; the words are the outpouring of that feeling as it issues not directly in prayer and praise but as it issues into words which reveal to us, and to Wordsworth himself, the thoughts or images which he had not in his mind, except as sublimated into the excitement which drive him into poetry. Images he may have had in abundance, but they were not images of the product; nor indeed could he have treated verse as a secondary adjunct if they had been. To speak still in general terms, so far as the poet works from images the poem is not the expression of *them*; so far as the poem evokes images in the mind they are revealed through the words.

It is sometimes said that the work of art is finished with the image in the mind which is at the same time the expression of the artist's mind, and embodiment in physical form is a matter of technical skill, required to communicate the work to others. So far as this statement can be construed consistently with facts, it raises a purely philosophical point, not a psychological one. The work of art may be called an image in the mind, if trees and men are also only mental. The philosophy appears to me a topsy turvy one. For it makes the physical work of art the mere publication of the artist's private possession; while the facts seem to show that the artist's image of his work of art, if he has one at all, is but the private copy of a public possession. For the facts, if I read them rightly, show that the product which is physical and public to begin with is the very means by which the artist himself discovers the state of mind which he wanted to express. But seeing that both physical product and its image are supposed alike to be mental, the doctrine interests us not as psychologists. What is important to retain is that it is only as colours or marble or words or musical sounds, whether presented in physical form or in the derived form of an image, that the artist comes into possession of himself and of his meaning.

What the exact character of the excitement is which is the proper impulse to creation, it may be difficult to say. It may be something of a shock to realize how lowly in character it may be. To judge from the glow that accompanies productive work of inferior kinds, such as everyone has experience of who tries to write a brief essay or even a letter and make the result as artistic as is open to him, the excitement is mainly a feeling of unrest, which keeps one in suspense, but a directed suspense like that we are aware of when we try to remember a name which we have forgotten but know that it is connected with this, that and the other circumstance and we feel ourselves straining towards it but unattaining. Over and above this, the impulse is felt mainly in bodily repercussions, in the visceral organs and the organs of secretion, about the heart and down the back and all over the body. It is a directed restlessness for it varies according to the topic, but what it is the indication of, what hidden movings urge us forward into the customary outlet of words or other forms of artistic material, is not disclosed until we have in semiblindness achieved the desired product.

The truth or untruth of this description of the process of artistic creation is only to be decided by the judgment of artists themselves, helped by observation of their behaviour in creation; and since it is the business of artists to create and not to psychologise, there may be difficulty in extracting their answer to questions which seem to them strange and perhaps unimportant or in the old sense of that word impertinent. That is why I am anxious to induce others to raise the question with artists in all kinds. I have myself made some headway with such an inquiry but at present not much, and I should welcome the collaboration of others if they should think the inquiry worth while. The question is really a simple one. In the stanza I quoted from Burns, I am suggesting that, while the poet is full of the passion of love and perhaps thinks of its continuance or even forms to himself an image of an unending love, an image drawn from the subject matter—that, as concerns the thrilling words of the song, "till all the seas gang dry," he has no image of those words or even probably of that reflection, but that the words (with their meanings, of course) are wrung from him by the effort to express his passion in words, and that the words when they are spoken reveal to him the everlastingness of the passion. Is this true or not? Or does he rather have an image in his mind of which these words are only a bare scientific record? I put the second alternative in this form, for of course a poet may have an image of the poem, may make up the poem in his head before he pronounces it aloud; which poets often do, but the fact has no interest. A painter may in image see his picture in the form it will appear when he has actually painted it; but the image is then only the image of the picture and no one contends that this is the regular procedure of the painter. So far as this happens the work of art is already made in image.

In testing the truth of the description I may also refer you to your own experiences as before. Do you or do you not, imagine beforehand everything you mean to say? Or is not your procedure to steep yourself in the subject matter, let it play in your mind and develop into all its suggestions and then when your excitement is at the full, let yourself go blindly, and then discover by success what you had in your mind to say?

An opportunity which I have recently had of observing an artist, and a great one, at work appeared to me to bear out my sketch of the creative process. The artist, a sculptor, was unacquainted with his sitter, could hardly have formed any preconceived idea of the kind of character he was trying to produce in the clay. He in fact denied that he did so set out but that his object was to study the form of his sitter's head in every detail. To judge from his behaviour which I was privileged to witness he was utterly absorbed with the head he was modelling, and his excitement about it was manifest at every stage—first, when he was sketching the head each successive bit of clay was dabbed on to the growing shape, as if he were indulging in a violent pugilistic encounter with it, a gymnastic display which testified to his intentness on the exact delivery of each increment; and later when, the general form being modelled, the smaller knife was used to perfect the detailed structure, here removing clay or indenting lines, here adding tiny portions of liquid clay with as much delicacy as the painter uses with his brush, the sculptor's face marked the strenuous effort to transfer his subject into the clay. And yet this meticulous study of the sitter resulted in no literal copy of him but was full of the artist's vividness and rather a reading of the original than a transliteration of it. Plainly it was not only the aspect of the head which worked in the artist's mind; these forms were moulded by him, as if they passed through an alembic. His eyes saw differently from another man's and his hand was obedient to his eye. Therein lies as I suppose the personality of the artist that he selects or adds or accentuates so as to bring out what is significant in the form. A direct answer to my question I could not elicit. But this particular artist seemed to think that both procedures might occur with him, both the semi-unconscious one I have suggested to be typical and the conscious forethought of some particular result, so far as he was guided by knowledge of the history of his art. My general impression was that I was witnessing absorbed observation of the sitter and that that observation, after filtration through what for want of a better word has to be called the artist's genius (for what this loose word means is mainly the unclear and undefined prompting of personality) ended in a creation which was far more significant than an ordinary observer like myself would have judged the original to be.

I have intentionally simplified my thesis and it is time to qualify it.

I am only suggesting it as an account of what happens in general and is the fundamental character of the artistic process. Even so far as I have gone in my inquisition it is clear that both processes are employed, execution by the relatively blind impulsion of the creative gadfly, and transcribing from pictures in the imagination. I set aside first, to repeat myself, those cases in which an artist of exceptional experience or skill produces his work in his head. Leonardo may have seen the picture of the Last Supper in all its details before he put the pigments on the wall; Wordsworth composed his poems on his walks, giving the simple peasants the impression of a mooning creature; the deaf Beethoven must have made the Choral Symphony in his imagination, and perhaps most musicians trust from the nature of the case to their imaginations. One eminent painter referred to in my correspondence formed so exact a picture in her mind of a portrait that when she came to put it on the canvas and desired to alter it, she found she could not. Is it not evident that the imagination in such cases is the work of art itself, and embodies the materials of the external work (I will not say the physical work) and possibly even the necessary movements of the hand. There is no more difference between it and the external work than there is between a sum done in the head or on paper. The question still arises how is this image reached? You would not say from a precedent image of itself. And if not then how? The case is irrelevant psychologically, for in such an image the work is already achieved.

I set aside such cases. Moreover even when the artist does not do the whole work in his head, he may do parts of it in this way, and tends to do so the more in proportion to his experience and his acquaintance with the history of his art. But there is another way in which he may use images, not of the finished product, but of the subject matter, realized in such detail that it may seem he is merely transcribing his picture into words or colours in the same sense as a man of science describes a natural phenomenon or an animal form. Perhaps it is such cases which are in the minds of those who say that the external embodiment in the material form is merely a matter of technique. In other words the artist merely describes in his customary medium a fully realized picture of his subject matter. The supposed case is not so likely to occur in painting or sculpture, for there the more vividly the artist pictures his subject, say his landscape, or the person he is modelling or sculpturing in stone, the more his image approximates to a picture of the finished product, and the situation is the one already mentioned. In poetry however the poet often seems to be merely describing, as it were scientifically, a picture in his mind. It may be doubted whether even in purely descriptive poetry he does so really, unless exceptionally. In general the picture and the words mould each other: the picture may be transcribed in words, but the words as they flow alter the picture. One of my correspondents urges that when Wordsworth wrote of the daisy

> The beauty of its star-shaped shadow thrown
> On the smooth surface of this naked stone

the picture preceded the words. *A* picture doubtless, but was even that not altered in the transcription? 'Smooth' and 'star shaped' seem to exceed the mere picture. When Tennyson writes

> The lizard with his shadow on the stone
> Rests like a shadow

the phrase "rests like a shadow" betrays the effort to turn the picture into words. If I am right even these pictures fall back into the rank of subject matter in which the artist is absorbed but whose real character is only fully revealed when the words are spoken (or imagined). The lines which immediately follow the words quoted from Tennyson are:

> The purple flowers droop, the golden bee
> Is lily-cradled.

The poet is working from a picture; but is it likely that the 'cradling' of the bee in the lily was part of the picture or suggested by a fresh one. It was wrung from him by his delight in his picture, and completes the feeling (to repeat Mr Montague) from which he worked.

Moreover, as the details are transcribed (and let it be granted they are literally transcribed) they affect and are affected by the rest of the composition, and either themselves must undergo change, or they alter the total. The sculptor I observed when he introduced a touch with his knife into the clay model's nose or eyes found he had affected the unity of his model and so he was always working over the whole head at once.

The artist proceeds by stages, filling up his general impression into fullness. Partly as I have said he works from new images in the manner conceded. Partly he works by what a correspondent calls *tâtonnement*, perpetually correcting the product. It may be asked, does not his correction show that the work achieved falls short of his ideal, of some image in his mind of the perfect work? The answer I suggest is that it falls short of his ideal but not of the alleged image. If he had that image in his mind, his failure could only be a matter of technical unskill. He goes on correcting because his work does not satisfy the impulse which drives him into his work, with all those complicated sources from which that impulse is drawn which I have so inadequately sketched.

But it is precisely because the artist's working is so various that I desire to have more data. The variations of procedure attested at first hand are certain to be of more importance than rough and possibly incorrect or at least unguarded dogmas laid down by a person who is not himself an artist. Such material would be the only satisfactory basis of a thorough psychology of the matter.

There remains a further problem which arises from the existence of that 'eidetic' imagery which has been so interesting and important a discovery of recent years. Children up to about fifteen in large numbers, and adults in much smaller numbers, appear to possess images of what they see which in some respects resemble the images of memory but in others resemble after-images. The image has the substantiality of a percept, the person sees the object or scene very much as if it were really before him; it has a hallucinatory character. Now it is natural to think that artists may be individuals who retain this gift which other persons lose after youth. "Many men," says Rossetti,

> Many men are poets in their youth,
> But for one sweet-strung soul the years prolong
> Even though all change the indomitable song.

And Mr Kroh who gave the first systematic account (not the first account) of these facts has made a study of certain German poets past and present, one of them being Goethe, and concluded on internal evidence that they were eidetics. It by no means follows that all artists are so; a passage is quoted describing Goethe's impatience with Schiller because when Goethe described the *Urpflanze* from a vivid picture which he had in his mind, Schiller, who perfectly understood him, called Goethe's topic merely a notion. Let us assume however for the moment that all poets and other artists have such pictures in their minds. It might well be asked, does the artist, does the poet for instance, do anything more than read off his image as a scientific man would read it off? I believe I have really answered the question in advance. The real problem would in the first place be to explain why the artist comes to possess such highly finished pictures; in other words the artistic creation lies behind the eidetic image used. In the next place, I incline to think that though eidetic artists have these vivid pictures which simulate reality these pictures are but an additional gift they possess in conceiving the subject matter. The well known case of Reynolds' friend who painted from a single sitting of his subjects is typical. The eidetic image took the place of the reality. There is even one case quoted of an engraver who projected his image on to the plate and merely traced the outline with his tool.

Whether the result was good or bad we are not told. But the case is surely exceptional. In general, when the artist paints from his image or describes it in words, he is moulding it, in the product itself, into something of which the image supplies him with the subject but whose complete idea is revealed in the product itself.

For completeness' sake, I will return for a moment to the organic character of the material work of art, confining myself to poetry. If the doctrine be correct, the mere sound of the words is vital to the art. Yet words have meaning and it is never possible to dissociate the meanings of words from the words themselves. Two conclusions follow which I may expound briefly by illustrations. In the first place, no subject however trivial or mean is beyond the reach of art. Success depends upon the handling, though no doubt the higher the subject the greater the work if the words are commensurate. To illustrate triviality of subject in a perfect poem I will cite a stanza from a well-known seventeenth century poem, hardly daring to quote it because under our modern conditions it requires an effort to realize the picture:

Her feet beneath her petticoat
Like little mice stole in and out,
    As if they feared the light.
And oh! she dances such a way,
No sun upon an Easter day
    Is half so fine a sight.

Miss Austen's novels are compact of delicate trivialities, and how great is their art!

Secondly, there is a regular adjustment of sounds to meaning, and verse is far from being a superadded charm, but is an integral, possibly the chief constituent. The burden of Mr Saintsbury's work on prosody is this theme. Metre and rhyme are not mere adjuncts of poetry. The periodic recurrence of sounds or groups of them is part of that process by which the unitary purpose of a work of art is effected in its material. We may take it that as Mr Middleton Murry says[1] poetry differs from prose by its greater, at least its intenser, passion; that passion finding vent in words is ordered and controlled by rhythm and metre and rhyme. But I am more concerned with the blending of the delight of sounds, isolated or in their periods or in their cadence, with the thought or subject matter. Indeed it is mainly through poetry that we realize the charm of words. I come myself from no mean city but, its name suggesting smoke and damp, few persons perhaps think of the word as charming.

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[1] In his book *Style*. Oxford.

They have only to repeat the words of the Jacobite song, "Farewell, Manchester, noble town farewell," to realize how beautiful a word it is. In good poetry there always is this consonance of thought and song. Where either element is found alone we have defect—the tinkle of words or the aridity of bare ideas. Illustration would be endless, but it would be taken, not from the deliberatively imitative passages, where sound of word recalls sound of nature, but from great music like that of Prospero's speech in *The Tempest* or the cadence of Cleopatra's "Give me my robe etc.[1]"; from the rolling catalogues of names in Milton, or the organ music which accompanies Adam's prayer, or the fragrance and softness of the words themselves describing Eden in Book v. I will cite two examples; the first, to reprove Wordsworth's doctrine by his own practice, shall be the famous skating passage in the *Prelude*:

> So through the darkness and the cold we flew,
> And not a voice was idle; with the din
> Smitten the precipices rang aloud;
> The leafless trees and every icy crag
> Tinkled like iron; while far distant hills
> Into the tumult sent an alien sound
> Of melancholy not unnoticed, while the stars
> Eastward were sparkling clear, and in the west
> The orange sky of evening died away.

The second where the purpose in the blending is more deliberate is from the stanza describing the concert in the garden of Acrasia in the second book of the *Faery Queene* where the unison of sounds is expressed by the linkage of the lines.

> The joyous birdes, shrouded in chearefull shade,
> Their notes unto the voyce attempred sweet;
> Th' angelicall soft trembling voyces made
> To th' instruments divine respondence meet;
> The silver sounding instruments did meet
> With the base murmure of the waters fall;
> The waters fall with difference discreet,
> Now soft, now loud, unto the wind did call;
> The gentle warbling wind low answered to all.

After this rambling discourse, which may have seemed less suited to a grave scientific society than might have been expected, I will put myself in order by a summary of what I have, not proved, for I have proved nothing, but suggested to stimulate further inquiry and discussion.

1. That art is purposeful, is expressive, is not purely descriptive like science and bound to its subject, is essentially communicative.

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[1] The remark comes from Mr Murry.

2. That art is an organic response as much as a perceptual response, but produces an object distinct from what provokes the response, and handles it for its own sake.

3. That the impulse to creation is based upon the material passions provoked by the subject, but is distinguishable from them and is formal.

4. That the process of creation arises from the excitement caused by the subject matter, and images play a large part in the awareness of that subject. These are not images of the work of art, which are in general only got through actual production.

5. That images of parts of the work of art may precede the actual production, but that this is a subordinate feature of the creative process. When there is complete anticipation in idea or image, the work of art has been already produced.

6. That eidetic imagery which is probably frequent in artists belongs to the images of the subject matter not of the product.

7. That the material of the art is a vital part of artistic process. In poetry for instance the sound of the words, their rhythm, metre, etc. are integral to the work, not an added charm, and possibly the element of prior value.

Perhaps after this dry statement of its contents, my discourse may seem more scientific than may have appeared upon its rhetorical surface.

(*Manuscript received* 7 *December*, 1926.)

MATERIAL *VERSUS* ABSTRACT FACTORS IN CORRELATION

BY C. SPEARMAN.

YET another paper has come from Professor Thomson based on Weldon's device of obtaining correlations by means of dice. Again he tries out the effect of arranging these dice according to 'chance.' And once more he devotes himself to attacking the school of research to which I belong[1].

But although not insensible to his compliment in so often noticing our work, still I would entreat him to pause a while and consider a little more whether he has rightly understood it. He complains himself that he is sick of polemics (though without letting this deter him from going on with them); and we for our part, too, are none too happy at being—as we think—time after time misrepresented.

The discrepancy between his account of us and our own is at once patent when he starts off by declaring that "the essential point" in our theory of correlations between abilities "is the absence or unimportance of group factors." Still more emphatically, he writes in another publication as follows:

> It is this absence of Group Factors which is in truth the crux of Professor Spearman's theory, which is not so much a theory of general ability, or a theory of two factors, as a Theory of the Absence of Group Factors[2].

Now, our real view—if we ourselves may be judges of it—is about as far from this as it could possibly be. We claim to have originally discovered group factors[3]. We maintain that we did so some twenty years ago. Beyond doubt, we have been sedulously investigating them ever since. And we believe ourselves to have actually traced out their presence throughout the whole length and breadth of human ability[4]. Whereas Thomson, to the best of my knowledge, has never investigated—if he has so much as mentioned—the nature of a single one of them!

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[1] This *Journal*, 1927, XVII.

[2] *The Essentials of Mental Measurement*, by Brown and Thomson. But this part of the book seems to represent only the views of Thomson.

[3] *Amer. J. Psych.* 1904. Also especially, *Zeit. f. Psychol.* 1906, p. 103.

[4] *The Abilities of Man*, by Spearman (Macmillan & Co., 1927).

What we have indeed said is that, under certain frequently occurring conditions, the criterion of 'tetrad differences' (or any of its earlier equivalent forms) has shown itself to be satisfied throughout the set of abilities at issue. We have asserted that, *when the criterion is thus satisfied*, then there can be no group factor of *appreciable magnitude*; that is to say, none exceeding the limits of the error of the experiment.

In part, this strange discrepancy in Thomson's account of our work may be traced to undue generalizing. He writes of group factors in such sweeping terms as have just been quoted, whilst really he appears to have had in mind only some group factors of a very special and peculiar kind. These are characterized by being extremely numerous in every ability, and therefore each having in that ability an extremely small influence; whilst their distribution among different abilities and individuals is conceived to occur 'by chance.'

But there is a further source of trouble, subtler than the preceding one, and correspondingly more dangerous. This derives from these peculiar group factors even when explicitly recognized as such. His own view would seem to be that *either* they, *or else* a general factor, will suffice to satisfy—approximately at any rate—the criterion of tetrad differences. Us, on the contrary, he depicts as holding only to the latter alternative and rejecting or overlooking the former.

In truth, however, even these multitudinous and minute factors appear to have been first suggested by ourselves[1]. And we have in some ways gone further about them than even Thomson. For whereas he again and again remarks that this or the other table of correlations has comprised too few individuals to distinguish the effect of the multitudinous group factors from that of the single general factor, we for our part have declared that *no* finite number of individuals, however large, could possibly secure any such distinction. And this may be affirmed, not only of the highly artificial arrangement of 'chance' as it has been and still appears to be employed by Thomson, but even of the perfectly natural arrangement made by Garnett.

Really, our position has been that, so far as the said criterion is concerned, the two contrasted cases—the multitude of group factors without any manifest general factor, and the single general factor without any manifest group factors—can quite well *both exist together*; they can constitute merely different aspects of the very same mental structure.

This can be demonstrated in many ways. To begin with, the most rigorous proof has been furnished by Garnett and myself[1] that whenever, and to the extent that, the criterion of tetrad differences is satisfied, then to this full extent the case of the single general factor *must* occur. Whenever, therefore, this criterion—as Garnett has proved it may be—is approximately satisfied by variables which to outer view present only the case of the said peculiar group factors; then inwardly these self-same variables must necessarily, and to the same degree of approximation, present also the case of the general factor. Garnett has even disentangled and plainly set forth the general factor in Thomson's own scores[2]. And as much can be done under every condition whatsoever, dice-throwing or otherwise, that can conceivably be invented to satisfy the criterion; every time, the general factor of every individual concerned can actually be measured[3]. When, then, Thomson still roundly and categorically asserts that his dice-scores possess no general factor at all, this I must —with Garnett—venture flatly to contradict.

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[1] The first mention of them was in *Psych. Rev.* 1914, p. 109.

And why in the world should not the two cases be mutually compatible? Why should not, for instance, the single general factor consist in some function of multiple group factors? The contingency that it might thus measure something not simple but complex was expressly stated at its very first enunciation[4]. Why should not even the hypothetical 'mental energy' itself be a function of very numerous and therefore very small factors distributed among abilities and individual persons by 'chance'? Such a proposition, far from being forthwith incredible, has at first sight much plausibility. And if for my part I have been obliged to reject it root and branch, this has assuredly been upon grounds quite other than the said criterion[5].

Now, according to Garnett—probably the finest mathematician among living psychologists—all the 'errors' and 'misapprehensions' of Thomson may be attributed to his "lack of definition of the terms employed[6]." But perhaps we may go still deeper and trace his inabilities down to a psychological source. He does but follow the general practice when he assumes that the correlation between two variables may be regarded as deriving from the fact of their possessing some elements in common. But then he seems unable to get away from the habit of taking such elements as being—like his throws of dice—hard material facts which cannot be converted into anything different from themselves. We do indeed know—thanks to Garnett—that every total variable can really and truly be expressed in terms of part-variables that are mutually independent; but we have no ground whatever for assuming that these part-variables are material facts or things incapable of being converted into anything else. In mathematics, at least, such conversion is among the commonest of operations. For example, if a, b, c, etc. can be expressed as depending on the independent elements, x, y, z, etc., this does not in the least preclude them from being expressed instead in terms of the quite different independent variables, p, q, s, etc. Similarly, x, y, z may be regarded as dependent on p, q, s, etc.; or *vice versa*. Or, indeed, either of these sets may be taken as dependent on a, b, c, etc. And if we ask how far such convertibility holds in the sphere of positive science, the answer must be that it certainly holds to a very large extent. Indeed, in some sense or other, the converted form of representation is *always* just as true as the original one (though it may or may not be of equal scientific usefulness). Consider even the position of a point in a plane; the independent part-variables of which this is a function may, with quite equal truth, be made to consist, either in its ordinate together with its abscissa, or else in its radius vector together with its vectorial angle. And such convertibility becomes even more pronounced on introducing the exceedingly abstract factors of biology or psychology. To take a homely instance, the excellence of a dinner may be said to depend, either upon the quality of the food together with that of the drink, or else—no less truly—on the amount of money spent, the judicious way of expending it, and the state of the market. It may be added that the possibility of g being *not* anything concrete, but something highly abstract, had been emphasized by our school long before any dice were brought on the scene.

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[1] Garnett, *Proc. Roy. Soc.* A, 1919. Spearman, *Psych. Rev.* 1920; *Proc. Roy. Soc.* A, 1922.

[2] This *Journal*, 1920, x, p. 251.

[3] *Footnote* 3, p. 322.

[4] *Amer. J. Psych.* 1904, p. 284.

[5] *Footnote* 3, p. 322.

[6] Expanding this sentence in one place, he says that: "It can only be the absence of these definitions that renders possible such a statement as that the deduction of 'The Theory of General Ability, or the Theory of Two Factors' from the occurrence of 'hierarchical' order among the correlation coefficients is invalid" (*Brain*, 1921–22, xliv, p. 334).

In consequence, perhaps, of Thomson's unfortunate induration of thought by the habit of dice-throwing, he seems to have never been able to realize that the view of our school constitutes a Copernican revolution. All other psychologists dealing with the topic have set out from some ill-defined mental power, such as 'intelligence,' and have sought to obtain a measurement of this. We, on the contrary, have started from a perfectly defined measurement, 'g,' and have been laboriously ascertaining what aspect of ability this g really measures.

By all means, then, let Thomson—with, or much better without dice—go on with his study of chance. It may yet prove quite helpful in suggesting the nature of the mental structure which underlies the g. But this g itself it can never stir by one hair's breadth. The two lines of investigation—on the one hand the establishment of g, and on the other the inquiry into the consequences of 'chance'—has each its own separate field. There need be no sort of conflict between the two—save a generous rivalry as to which can become the more useful.

Unfortunately, however, matters are complicated by the fact that Thomson, in this last paper of his, has slipped into a very serious new pitfall. He has replaced all my graphs of the tetrad differences by new graphs which he has calculated for himself. The result is that now half of these differences are altogether left out! By this mishap, his graphs have become distorted beyond recognition, his controversy resting upon them is left in the air, and I am anxious lest their publication should prove to be another set-back to the progress of psychology[1].

[1] Apparently, he has in each tetrad of correlations (as ordinarily written) taken only the product of the high left and low right values minus the product of the high right and low left values. Everywhere he has overlooked the converse difference, namely, the product of the high right and low left values minus the product of the high left and low right values.

Matters would not have been so bad, if he had quoted only the absolute values of the tetrad differences which he did take into account. For then at any rate he would have supplied a correct picture of one wing of the frequency distribution. But instead of this, his procedure was such as to take a portion of these differences with the plus sign and the remaining portion with the minus sign. The choice as to which should be which depended quite arbitrarily upon the special order in which the table of correlations happened to have been written out. The result was to render his frequency distribution as far as possible asymmetrical. Whereas the true complete distribution cannot help being, on the contrary, altogether symmetrical.

(*Manuscript received* 22 *February*, 1927.)

A GROUP FACTOR IN SENSORY-MOTOR TESTS

By ERIC FARMER

1. *Earlier work* (p. 327).
2. *Results obtained from tests of aestheto-kinetic coordination* (pp. 327, 328).
3. *Criticism of earlier work* (p. 329).
4. *The theory of a general aestheto-kinetic factor* (pp. 329–332).
5. *Relation of these results to vocational testing* (pp. 332, 333).

1. EARLIER WORK.

THE history of sensory-motor tests has been a somewhat chequered one. In the early days of experimental psychology much attention was paid to them but the results yielded seemed to be of little value, and psychologists turned their attention more and more to 'mental' tests. More research has been done on this particular aspect of applied psychology than on almost any other and the result has been that comparatively reliable intelligence tests have been constructed which are being used extensively in practical life. One result of the research on mental tests on which most experimenters are agreed is that the intercorrelation between intelligence tests is high and this has led a large number of psychologists to the conclusion that intelligence is a general factor.

Both Perrin (1) and Muscio (2) have carried out experiments with a view to seeing whether any similar general factor shewed itself among motor tests and they both came to the conclusion that there was no general factor of motor dexterity. In my work for the Industrial Fatigue Research Board during the last four years I have had occasion to give motor tests to a large number of subjects and the data thus obtained has led me to the conclusion that the views of Perrin and Muscio need modification if they are adequately to interpret the whole of the available data.

2. RESULTS OBTAINED FROM TESTS OF AESTHETO-KINETIC COORDINATION.

I will first state the results of my own work and will then return to that of my predecessors in this field of research and see how far their results are at variance with mine. For the sake of clearness I shall not refer to the tests I have used merely as motor tests but as tests of aestheto-kinetic coordination. The term motor is too wide and must include all tests involving movement; by aestheto-kinetic tests I mean a special group of motor tests which have the common characteristic that afferent impulses received through the sensory organs have to be interpreted as signs for the performance of certain rapid and accurate movements by the hand or foot. In order that these tests may be successfully performed, the afferent impulses and the muscular movements of which they are the signs must be accurately coordinated. The aestheto-kinetic tests with which I am most familiar are the Choice Reaction test, the McDougall-Schuster Dotting test, and the Pursuit Meter[1]. These tests have been given to several groups of varying sizes and their intercorrelation coefficients are shown in the Table. Every test has not been given to each group, but in each case the number of subjects tested is sufficient to obtain a really representative correlation coefficient. The average correlation coefficient between the three aestheto-kinetic tests is ·25. This is certainly small, but the consistency shown between the intercorrelations in the various groups and the number of subjects tested make it significant. The average intercorrelations between the aestheto-kinetic tests and intelligence is ·05 and between reaction and the dynamometer is ·02, which shows that they have no relation whatever with the qualities measured by an intelligence test and by a dynamometer. The average of the coefficients of correlation between the Number Setting test and the Reaction and Dotting tests is ·11, which is certainly small but not altogether negligible, and its average correlation with intelligence is ·21. This test was designed in order to measure intelligence expressed through a mechanical rather than a scholastic medium. The fact that it correlates to a small degree with two of the aestheto-kinetic tests and also with intelligence seems to indicate that the purpose for which it was designed has been at least to some extent realized.

The conclusions to be drawn from these results are:

(1) That there is a small common element among the aestheto-kinetic tests used which shows itself consistently in all groups. This common element is not so marked as that which connects intelligence tests and it is not due either to intelligence or strength since there is no correlation between these factors and the aestheto-kinetic tests. Nor is it connected with temperamental instability as measured by tremor or defective muscle balance.

(2) That the Number Setting test is a connecting link between intelligence and aestheto-kinesis since it correlates in a small degree with both, though they do not correlate with each other, and it may be an indication of another group factor as yet unexplored.

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[1] For description of these tests see *A Psychological Study of Individual Differences in Accident Rates*, by E. Farmer and E. G. Chambers, Report No. 38, Industrial Fatigue Research Board, H.M. Stationery Office, London, 1926.

3. CRITICISM OF EARLIER WORK.

Let us now return to the work of Perrin and Muscio and see in what way their conclusions are at variance with mine Both of these workers experimented with the object of seeing if they could find any indications of a general motor factor. Since this was their object, the tests they employed were a diverse group of tests having very little in common except that they were all motor tests. It would indeed be surprising if tests of such a diverse nature intercorrelated highly. The term motor is too wide and we cannot expect to advance our knowledge of motor ability unless we are prepared to take narrower views of motor tests and attempt to divide them according to the particular type of motor performance they seem to test. Our knowledge of non-motor tests would not have advanced very far unless attempts had been made to divide them into intelligence tests, scholastic ability tests, imagination tests, etc. By dividing in this way we separate various psychological qualities and group the tests accordingly. The aiming and tapping tests which Muscio used seem to require a somewhat similar type of motor activity for their performance, and the weighted average of their intercorrelation coefficients is ·19. The fewness of the number of subjects Muscio dealt with would make such a small correlation coefficient insignificant, whereas the coefficients of a somewhat similar magnitude obtained in my work are significant on account of the larger numbers tested. There seems therefore no divergence between the conclusions of Perrin and Muscio and those I have suggested because the subject matter with which they deal is different from mine. They arrived at the conclusion that there was no general motor factor. I have not arrived at the conclusion that there is such a factor, but merely that certain motor tests which I have called aestheto-kinetic tests intercorrelate consistently with each other and not with certain other tests, and therefore I draw the conclusion that these tests have some element in common.

4. THE THEORY OF A GENERAL AESTHETO-KINETIC FACTOR.

It would be unwise at this stage to offer too definite a theory to explain this and it is better to examine various explanations that suggest themselves.

The fact that the intercorrelations between aestheto-kinetic tests is small may be because the tests used are not yet sufficiently developed. In the early development of intelligence tests knowledge was not always recognized as different from intelligence with the result that the intercorrelations between the tests were not so high as those generally found between intelligence tests now. It may be that some similar unknown factor is operating in the aestheto-kinetic tests and thus masking the true intercorrelation between different forms of aestheto-kinetic coordination. This point can be decided only by experimenting with new and possibly better tests, and seeing if the magnitude of their intercorrelations differs from that of the intercorrelations of the tests already used.

The smallness of the correlations may be a real indication of the degree of relationship between aestheto-kinetic performances and not due to the imperfections of the present tests. If this is so, we have the indications of a rather interesting psychological classification. Tests measuring intelligence, the highest and biologically the latest of our psychological functions, on the whole correlate highly. General motor tests which measure movements of various kinds do not correlate. These movements are among the oldest of our inherited capacities, and probably represent a large number of highly specialized motor activities each one of which was perhaps the only one possible to certain lower forms of life. When however we move up one step in motor capacity and confine ourselves to aestheto-kinetic tests which require a high degree of coordination and mental control for their performance, we find that these tests have an intercorrelation of about ·25. This degree of intercorrelation is intermediate between that of motor tests in general, which do not intercorrelate at all, and intelligence tests which intercorrelate highly. Coordinated movement requiring conscious direction is biologically intermediate between the ability merely to move and the ability to think. This same order is observed in the effects of an anaesthetic. First the higher centres of thought are affected, then highly coordinated movements such as balance and walking, and finally any movements at all, the pure reflex movements being affected last. The process of anaesthetizing proceeds backwards through our biological history and attacks first the functions that have high intercorrelations, next those which have small intercorrelations and finally those which have no intercorrelations. This parallelism between the magnitude of the average intercorrelation coefficients and the biological level of the functions from which they are obtained is curious, and if it ultimately turned out to be a true relationship, it might lead to a useful classification of psychological tests.

Another interpretation may be that there are certain group factors in sensory-motor activity, one of which is the common factor tested by aestheto-kinetic tests. Other group factors may show themselves as research work continues with motor tests. The intercorrelations manifested by these other factors may be greater or less than those found for aestheto-kinetic tests and the results may or may not fit into the biological interpretation suggested in the previous paragraph. Connecting links between non-intercorrelating functions may be found, such as the Number Setting test which is related both to intelligence and aestheto-kinesis, though they themselves appear to have no relation. By paying attention to the intercorrelation between different types of motor tests we may be able to make a more systematic psychological examination of human nature than has hitherto been possible.

The correlation coefficients yielded by intelligence tests both with themselves and with such measures as examinational order have on the whole been high, but this is no excuse for disregarding small significant correlation coefficients in other fields of psychological research. A small correlation coefficient, provided it is significant, tells us that two variables are related in a small degree with one another. A fact of this kind may be very important and to pass it by may seriously curtail psychological research.

Among young children fairly high correlations have been found between certain motor tests and tests of intelligence, but these intercorrelations tend to become smaller as the age of the children increases. The explanation of this appears to be that motor tests for young children are not really tests of motor capacity but of intelligence, since with a partially developed intelligence it is only the really intelligent children who understand what is required of them in a motor test. As specialization increases this ceases to be the case and the fact that motor tests no longer correlate with intelligence shows that they have ceased to be tests of intelligence and have become, as they were intended to be, tests of motor capacity. Among my subjects there is no correlation between motor tests, tremors, and defective ocular balance, whereas with young children there is usually a fairly high intercorrelation between them. This is possibly another example of the process of specialization which attaches a defect to some special function instead of allowing it to operate generally.

The average intercorrelation between the different questions in the intelligence paper used with my subjects was ·39, whereas the usual correlation coefficient between the questions of an intelligence paper for school children is not lower than ·50. This tendency towards a lower intercorrelation with older people even in an intelligence test may likewise be due to specialization.

This difference in the intercorrelation coefficients obtained from different age groups is sufficient to put us on our guard against accepting such correlations as real indications of psychological relationship outside the special sphere from which they were obtained.

5. RELATION OF THESE RESULTS TO VOCATIONAL TESTING.

Muscio's object in experimenting with the motor tests he used was to see if a general motor factor showed itself which might be useful in vocational psychology, and the results obtained rightly led him to abandon that method and to pay attention to more specialized tests which definitely attempt to imitate certain aspects of a particular industrial occupation. There can be little doubt that he was right in doing this, for even if we grant a common factor in aestheto-kinetic tests it is obviously not so important as the factors which differentiate the tests. Better results are likely to be obtained in vocational psychology by imitating certain occupational performances than by tests seeking to measure some general factor.

Whether the aestheto-kinetic tests do correlate with any form of industrial proficiency is at present uncertain, but in a few years' time it may be possible to correlate the performances of most of the subjects I have dealt with with some measure of industrial proficiency at the end of their period of apprenticeship.

In certain branches of vocational psychology we may not be able to make use of 'analogous' tests on account of the highly differentiated nature of the occupation we are considering. Engineering is an example of a profession which calls for the use of so many special abilities as to offer little scope for 'analogous' tests. If however some general factor is found, even if it is only operative in a low degree in the tests which presume to measure it, it may be of use. Aestheto-kinetic tests have been found to have a relation to accident proneness in all the different occupations of the subjects tested. It is however possible that 'analogous' tests might have had a more marked relation to accident proneness in particular occupations than that shown by aestheto-kinetic tests throughout all the occupations. At present we do not know whether this is so or not, but even if it is, the fact that those who fail in the aestheto-kinetic tests also tend to have an undue number of accidents in various occupations is of practical value. It may not always be possible to test each individual for each occupation he takes up during his lifetime, but if it is known that he has a general tendency to a high accident rate this is at least sufficient to turn him from seeking employment in a peculiarly dangerous trade.

Table of Intercorrelations between aestheto-kinetic tests.*

Tests	Groups	Nos.	r	
Reaction and Dotting	A	57	·35	
	B	100	·23	
	C	94	·22	
	D	175	·21	
	E	279	·26	
	F	17	·54	
	G	23	·33	Weighted av. r=·25
Reaction and Pursuit Meter	A	57	·24	
	B	100	·09	
	D	175	·17	
	E	279	·22	Weighted av. r.=·19
Dotting and Pursuit Meter	A	57	·39	
	B	100	·28	
	D	175	·21	
	E	279	·35	Weighted av. r''=·30
Reaction and Intelligence	D	175	·12	
	E	279	·08	
	H	524	·06	Weighted av. r=·08
Dotting and Intelligence	C	94	·16	
	D	175	·00	
	E	279	−·02	Weighted av r=·02
Pursuit Meter and Intelligence	D	175	·09	
	E	279	·01	Weighted av. r=·04
Number Setting and Reaction	D	174	·14	
	E	279	·12	
	H	524	·12	Weighted av. r=·12
Number Setting and Dotting	D	175	·08	
	E	279	·11	Weighted av. r=·10
Number Setting and Pursuit Meter	D	175	·02	
	E	279	−·03	Weighted av. r=−·01
Number Setting and Intelligence	D	175	·23	
	E	279	·21	
	H	524	·20	Weighted av. r=·21
Dynamometer and Reaction	H	524	·02	
Dynamometer and Number Setting	H	524	−·04	
Dynamometer and Intelligence	H	524	·03	

* Since writing this paper a group consisting of 449 subjects has been given the Dotting test together with three new aestheto-kinetic tests and the results are in entire agreement with those set forth in the table.

Table of weighted average correlations.

	Reac.	Dott.	Purs.	No. sett.	Intel.	Dynam.
Reaction	—	·25	·19	·12	·08	·02
Dotting	·25	—	·30	·10	·02	—
Pursuit	·19	·30	—	−·01	·04	—
Number Setting	·12	·10	−·01	—	·21	−·04
Intelligence	·08	·02	·04	·21	—	·03
Dynamometer	·02	—	—	−·04	·21	—

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1. Perrin, F. A. C. "An experimental study of motor ability." *J. of Exper. Psy.* 1921, iv, 24–56.
2. Muscio, B. "Motor Activity with special reference to Vocational Guidance." *Brit. J. of Psy.* 1922, xiii, Part 2.

(*Manuscript received* 13 *September*, 1926.)

PARALLELISM IN CURVES OF MOTOR PERFORMANCE

By ERIC FARMER.

1. *Tests used* (pp. 335–338).
2. *Results obtained* (p. 338).
3. *Determination of the length of particular responses irrespective of their position in the series* (pp. 338–340).
    (*a*) *Choice reaction* (p. 338).
    (*b*) '*Complications*' (p. 339).
4. *Evidence from other sources* (pp. 340–342).
5. *Conclusions* (p. 342).

1. Tests Used.

A good deal of attention is paid at the present time by those interested in educational and industrial psychology to the problem of the acquisition of motor skill, and it is certainly most important to understand the principles underlying acquisition of such skill, for failure to do so is in part responsible for some of the unwise methods adopted when training beginners. The present paper deals with one particular aspect only of motor performance but the data are sufficiently extensive and consistent to justify a considerable degree of confidence in the reliability of the conclusions.

The data were obtained from a series of choice reaction time experiments given in the course of a research undertaken by the Industrial Fatigue Research Board. Eight groups, comprising 1342 individuals, were given a choice reaction test consisting of thirty stimuli which were presented in the same order to every subject. The average time of response for each group to each stimulus in the series was calculated and the data thus obtained were plotted in graphic form. These graphs are shown in Fig. 1 and it is quite clear that they bear a remarkable similarity to one another. The form of each curve shows a gradual lessening in the time taken to react which represents the improvement effected during the test, but the most interesting thing about these curves is that the responses tend to follow a definite rhythm of long and short reaction times and that these long and short reaction times show themselves in

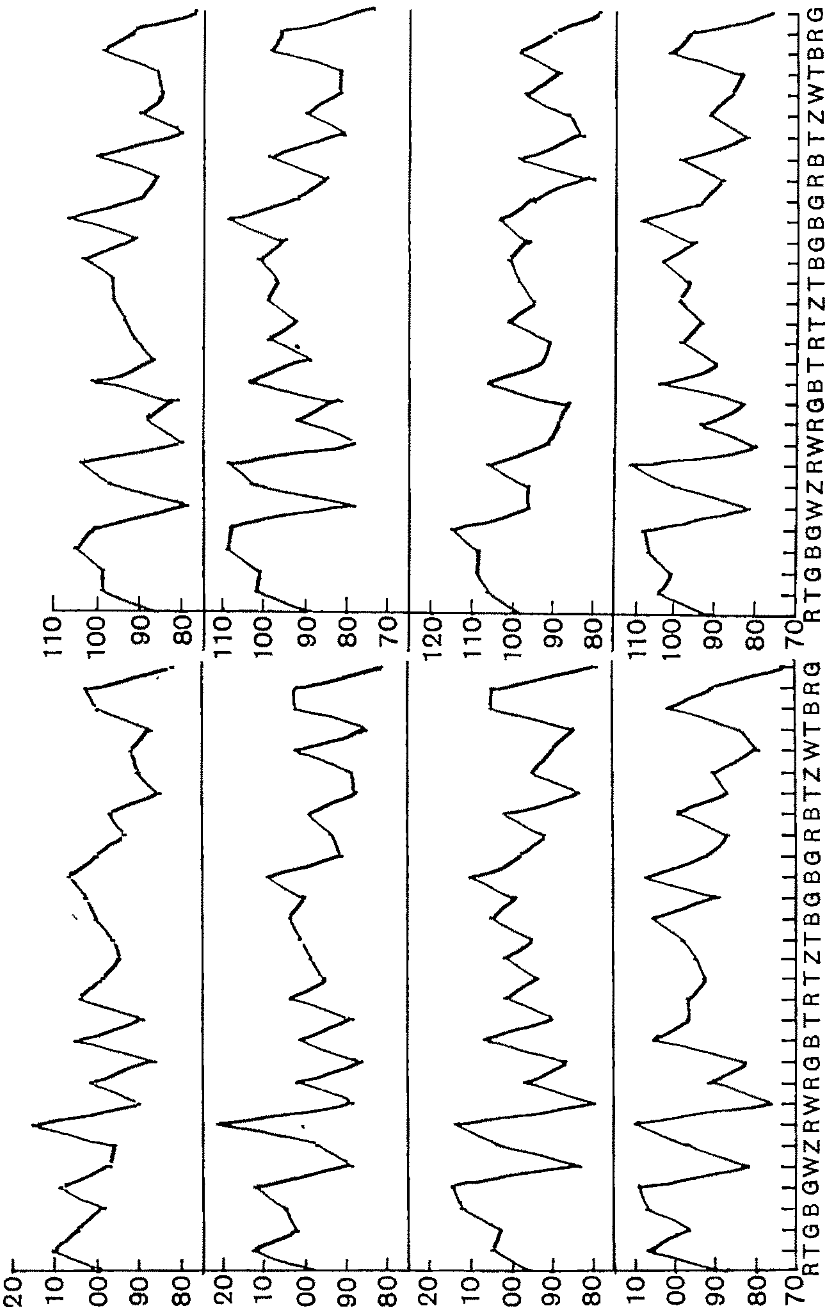


Fig. 1.

The letters refer to the stimuli: R = red, G = green, W = white, B = bell, Z = buzzer, T = touch.

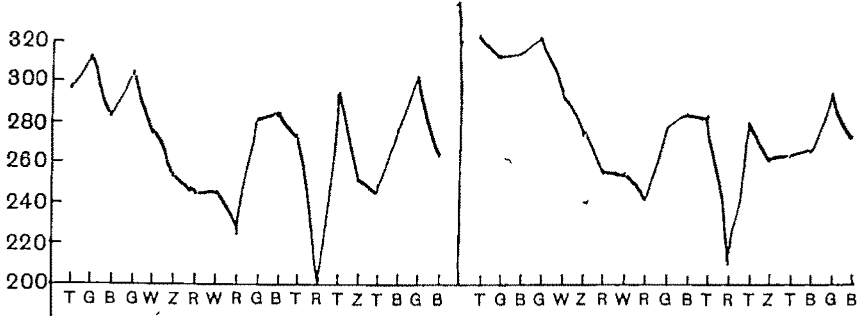
the same responses for each group. The intercorrelations between the 30 responses are given in Table I where it can be seen how high the coefficients are, all of them being significant.

TABLE I. *Intercorrelations of choice reaction times to each of the* 30 *stimuli.*

Group	A	B	C	D	E	F	G	H
A	—	·898	·861	·783	·777	·786	·821	·777
B	·898	—	·823	·737	·700	·748	·789	·698
C	·861	·823	—	·901	·909	·750	·970	·908
D	·783	·737	901	—	·918	·769	·934	·909
E	·777	·700	·909	·918	—	·760	·957	·954
F	·786	·748	·750	·769	·760	—	·782	·746
G	·821	·789	·970	·934	·957	·782	—	·939
H	·777	·698	·908	·909	·954	·746	·939	—

All these coefficients are significant.

Fig. 2.



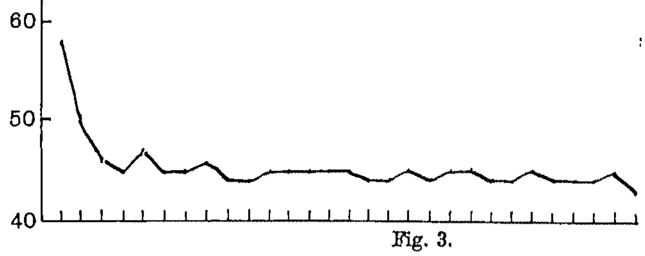


Fig. 3.

A different kind of choice reaction test was given to two of the groups and was called 'complications.' In this test the subject instead of responding to the stimulus by pressing its appropriate response key had to press the response key of another stimulus indicated on a chart in front of him.

In the ordinary choice reaction tests the mental process a subject had to perform consisted of (1) perceiving a particular stimulus, (2) locating its appropriate response key and performing the motor act of pressing it. In the 'complications' test the mental process consisted of (i) perceiving the stimulus, (ii) deciding either by memory or by looking at the chart which response key should be pressed, (iii) locating the proper response key and pressing it. The average reaction time for this test was about three times as long as that for ordinary choice reaction, and the curve of performance was of a different shape (see Fig. 2). The correlation between the two curves is high ($r = \cdot 923$) as in the case for choice reaction.

A simple reaction test was also given to one group. In this test the subject merely had to raise his finger on perceiving the stimulus. Fig. 3 shows the curve of performance.

2. RESULTS OBTAINED.

The frequency curve of distribution was normal for all groups in all three tests, showing that individuals differed greatly in the average time they took to react. They differed in their average performance and were only similar in the shape of their performance curves.

From these data we may draw the following conclusions with some certainty:

(1) That the performance curves of all groups in choice reaction are of the same form.

(2) That the performance curves for the two groups who did 'complications' are of the same form.

(3) That the shape of the curves in different types of reaction is different.

(4) That improvement is more gradual in the tests requiring choice than in simple reaction.

3. DETERMINATION OF THE LENGTH OF PARTICULAR RESPONSES IRRESPECTIVE OF THEIR POSITION IN THE SERIES.

(*a*) *Choice Reaction.* The average length of the response to each of the six different stimuli in choice reaction was calculated for each group and the results are given in Table II. The only marked difference in length of response is that the reaction to the bell stimulus is the longest in every group except two. It is also interesting to note that there is a smaller variation in the responses to the visual than to the non-visual stimuli.

The visual stimuli were given by raising coloured discs placed immediately in front of the subject. The three discs were white, green, and red. They were in the same position for all subjects and the position of their response keys could be changed. When they were on the left of a semicircle of equidistant response keys (groups A, B ,C, D, E, G, H) the white stimulus, which was on the extreme left of the visual stimuli, has the shortest response time in every case. In group F the visual response keys were on the right and in this group the red stimulus, which was on the extreme right of the visual stimuli, has the shortest response.

Table II. *Average length of response to each of the six stimuli in choice reaction for each group*

Group	Red	Green	White	Bell	Buzzer	Touch
A	103	95	93	102	94	95
B	103	97	93	101	93	94
C	101	96	84	107	100	92
D	93	90	79	104	94	94
E	91	89	80	103	95	90
F	92	96	95	102	92	95
G	95	92	79	103	97	91
H	93	87	79	106	100	90
Weighted average	95	94	87	103	95	93
Range	12	10	16	6	8	5

(*b*) *Complications*. The order in time of response in the 'complications' test was (1) red, (2) buzzer, (3) white, (4) touch, (5) bell, (6) green; the response to red being the shortest and that to green the longest. The chart placed before the subjects in this test to show which response had to be made to any given stimulus was in the following form:

Red means White
White ,, Bell
Bell ,, Touch
Touch ,, Green
Green ,, Buzzer
Buzzer ,, Red.

It will be seen that the shortest response is at the top of the list, the second shortest at the bottom, the third second from the top, and the fourth, fifth, and sixth in the centre. It is quite clear therefore that the main factor in determining shortness of response in both choice reaction and 'complications' was positional relationship and that extremes either right or left, or top and bottom are the easiest positional relationships to react to.

The coefficient of variation for each of the 30 stimuli was calculated for choice reaction in groups A and B, and for 'complications' in group G. The range of these coefficients was in the case of choice reaction 23·31, and of 'complications' 26·95, and their standard deviations, 2·72 and

5·49 respectively, show that the coefficients of variation differ very little for each response. It is clear therefore that the performance curves of individuals within each group correspond fairly closely to the group curve.

The number of mistakes made in responding to each stimulus was correlated with the average time taken for correct response to that stimulus and it will be seen from Table III that the coefficients on the whole are high and in each group except B significant. This shows that where particular responses are unduly long, more mistakes are also likely to be made. We cannot infer from this that there was any greater objective difficulty in the response, for the times shown on the curves of performance alternate between long and short irrespective of the nature of the particular stimulus to which response had to be made.

TABLE III. *Correlations between mistakes and time in choice reaction.*

Group	Number	*r*
A	57	·581
B	100	·288
C	94	·639
D	89	·681
E	85	·615
F	524	·487
G	279	·653
H	114	·532

All these coefficients are significant except that for B.

4. EVIDENCE FROM OTHER SOURCES.

The parallelism shown in the performance curves of choice reaction and 'complications' also shows itself in other performance curves obtained from different sources.

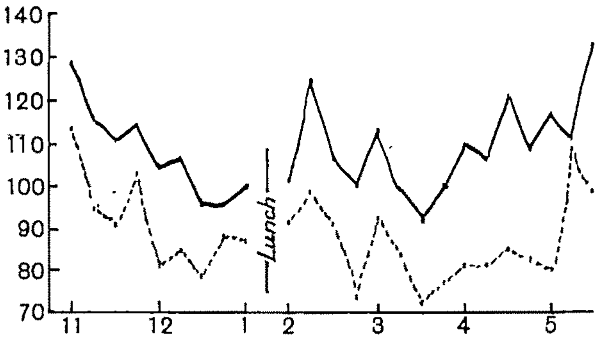


Fig. 4.

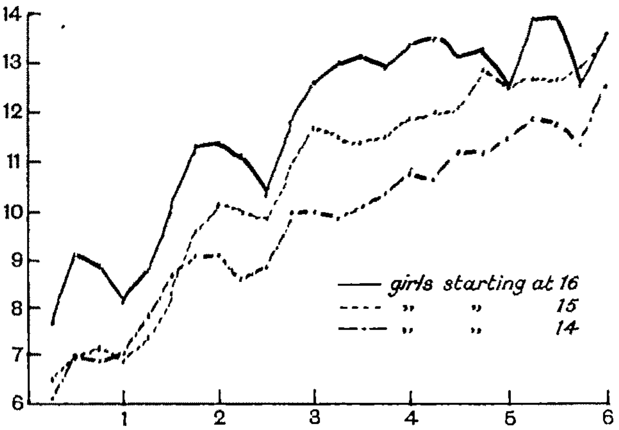

14
13
12
11
10
9
8
7
6
1
2
3
4
5
6
girls starting at 16
" " 15
" " 14

Fig. 5.

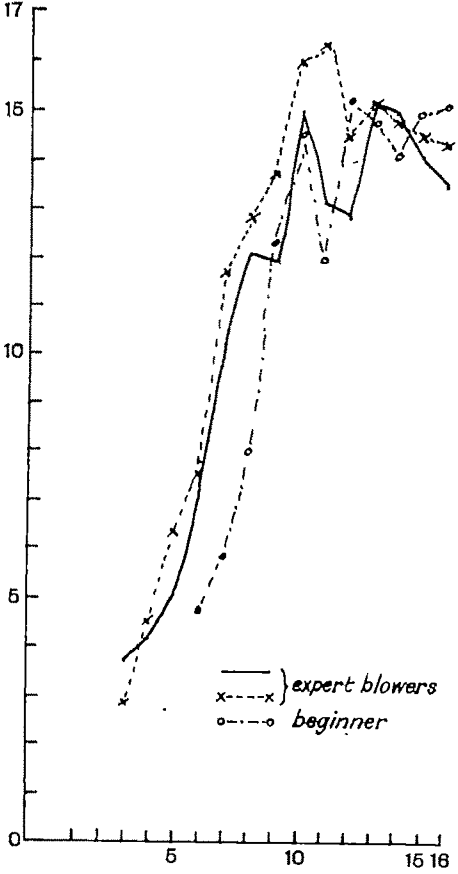

17
15
10
5
0
5
10
15 16
expert blowers
beginner

Fig. 6.

(1) Fig. 4 shows the curves of performance of two subjects doing a choice reaction-time test every quarter of an hour during the day. It will be seen that except for the end spurt manifested by one subject in both morning and afternoon spells the curves tend to be parallel.

(2) Fig. 5 shows the improvement curves of a group of girls employed in a simple industrial repetition process divided according to the age at which they commenced learning. Here too the same parallelism shows itself over a period of six years.

(3) Fig. 6 shows the improvement curves of three glass blowers, two of whom were expert and the other a beginner, starting work on a new type of bottle. Here again the curves tend to be parallel during the learning process. (See Report No. 24, Industrial Fatigue Research Board.)

5. CONCLUSIONS.

From the foregoing data we can draw the following general conclusions:

1. That curves of motor performance, whether derived from psychological tests or industrial work, tend on the whole to be parallel. In my research work I have come across no exceptions to this rule. This parallelism in curves of performance is extremely important from the point of view of industry, for it means that any improvement made in an industrial task with a view to making it less difficult will tend to affect all workers in the same way, though not necessarily to the same degree. The whole of the above data goes to prove that individuals are more similar than dissimilar, not in their actual performances but in their method of reacting, and since in industry we must always deal with groups more than with individuals this has an important bearing on the efforts made by psychologists to improve working conditions.

2. The fact that quickness of reaction is closely related with positional association is important from the point of view of machine design with a view to placing the controls in such a position as to facilitate easy manipulation. It has already been shown that slowness of reaction is one of the factors involved in accident[1] causation and we may therefore expect that machines which are designed in such a way as to facilitate quick reaction will, other things being equal, tend to cause fewer accidents than those which are designed without reference to this factor.

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[1] *A Psychological Study of Individual Differences in Accident Rates*, by E. Farmer and E. G. Chambers. Report No. 38, Industrial Fatigue Research Board, H.M. Stationery Office, London, 1926.

(*Manuscript received* 13 *September*, 1926.)

THE RELATION BETWEEN INTELLIGENCE AND SOCIAL STATUS AMONG ORPHAN CHILDREN

BY D. CARADOG JONES AND A. M. CARR-SAUNDERS.

(*From the School of Social Science, University of Liverpool.*)

I. *Introduction* (pp. 343–345).
II. *Methods* (pp. 345–347).
  Nature of schools tested (pp. 345, 346).
  Grading of social class (pp. 346, 347).
  Testing of Intelligence (p. 347).
III. *Results* (pp. 347–361).
IV. *Discussion* (pp. 361–363).
V. *Conclusions* (p. 363).

I. INTRODUCTION.

A NUMBER of investigations have been made which, as the result of applying intelligence tests to children coming from different social strata, show that the performance of children coming from 'lower' strata are not as good as those of children coming from 'higher' strata. This phenomenon was first examined by Binet and Simon (1) who worked with material obtained by Decroly and Degand in Brussels. The subsequent investigations may be divided into (*a*) those which classify the children according as to whether they live in 'good' or 'bad' parts of the town, go to schools of a 'superior' or 'inferior' kind, or come of rich or poor parents and (*b*) those which classify them according to the occupations of the father.

Among (*a*) may be mentioned the work of Burt (2) and English (3) on schools catering for different social classes in Oxford, of Bridges and Coler (4), Burt (5), Yerkes and Anderson (6) and Morlé (7) on schools in 'good' and 'bad' parts of towns, Arlitt (8) and Terman (9) on children from 'upper' and 'lower' social classes and of Kornhauser (10) on children of parents classified on a basis presumably indicative of income. In all these cases it is the children of the upper class and richer parents who come out best. Among (*b*) may be mentioned the work of Pressey and Ralston (11) and of Duff and Thomson (12). Pressey and Ralston tested 548 children between the ages of 10 and 14 who were divided into four classes according as to whether the occupation of the fathers was professional, executive, artisan or labouring. The average score of the children of professional fathers was found to be twice as large as that of the children of labouring fathers, the average scores of children of executive and artisan fathers taking intermediate places. Duff and Thomson analysed tests taken of some 13,000 children in Northumberland schools over 11 and under 13 years of age. They grouped the children into thirteen classes according to the occupation of the parents and found the average Intelligent Quotient to vary from 112·2 in the case of the children of professionals to 96·0 in the case of the children of the fathers in lower grade occupations.

These and other similar investigations which are in general all concordant show that on the average children of 'lower' status do less well in these tests than do children of 'higher' status. Various problems suggest themselves and among them one of particular importance towards the solution of which the investigation recorded in this paper was planned as a contribution. Do the different out-of-school experiences of the groups of children account in whole or in part for the differences exhibited by them in the tests? Would there be like differences in nature and degree exhibited by children whose out-of-school experiences are not different? The question might be tested by examining children who have been brought up together in orphanages where they are classified according to the former occupations of their fathers.

Investigations have previously been made upon orphan children which, though not similar to those recorded here, bear upon the problem. Schmitt (13) used the Binet-Simon tests upon 100 orphan children in Würzburg aged 4 to 16 drawn from the lower social classes. The average length of residence in the orphanage is not given but appears to have been about 5 years. His chief conclusions were that a large proportion of the children were retarded, that the retardation became more marked with increase in age, and that length of residence had no effect upon intelligence. He inferred that the inferiority of the children was innate and that the uniform environment had no effect upon intelligence.

Dr Kate Gordon (14 and 15) has published two investigations into children in Californian orphanages, the first of which has been dealt with by Pearson (16) and the second by Elderton (17). In the first investigation Miss Gordon reported the result of testing 91 pairs of siblings in three orphanages. Pearson using these data found a correlation of ·51 between siblings in respect of intelligence. Since this is almost exactly the same degree of association as is found between siblings chosen at random (each pair of whom therefore had not necessarily been at the same school), Pearson concluded that there was no sign here that similarity in environment had any effect on intelligence. In her second report Miss Gordon gave the results of testing 216 pairs of siblings. She calculated the correlation between siblings in respect of intelligence and found it to be ·61. This result would seem to indicate an increase in resemblance between siblings under similar conditions. Her data were examined in the Galton Laboratory. It was observed that Dr Gordon when considering families with more than two children never used the same child twice. Thus families of three yielded only one, pair the two chosen being next to one another in order of birth. When all possible pairs were taken the correlation was found to be ·467. The explanation appeared to be that these tests showed a gradual decrease in intelligence with increase in age and that therefore when all pairs are taken and not only those close together in age the correlation is automatically decreased. Miss Elderton concludes that "orphanage siblings resemble one another about as closely as children whose home conditions vary in a more marked degree."

The following investigation attacks from a different angle what is fundamentally the same problem.

II. METHODS.

Our purpose was to find out, if possible, whether children in orphanages who had been therefore subjected to the same sort of environment for some years still showed that differentiation of intelligence according to social origin which other workers, as stated above, claim to have observed in children living at home and therefore subjected to different kinds of out-of-school environment. It was hoped that some light might be thrown on this problem by testing a sufficient number of children who had sprung from different social grades but who had subsequently resided for an appreciable length of time in the same institution or institutions of the same class. In that way a step would be taken towards the disentangling of the two factors, heredity and environment, in the determination of intelligence.

Nature of Schools tested.

As will appear in the course of the paper, eight schools were visited altogether. Two of them, *D* and *F*, were industrial schools, containing boys who had in some way got into the hands of the police; they may have actually broken the law or they may merely have been found wandering or living with parents who had proved themselves unfitted to have the care of children. The other six schools were orphanages; one of them, *E*, was under Catholic control and the majority of the boys in it had been admitted through the medium of the Poor Law Authorities; another, *H*, at the opposite extreme, catered for a better social class and differed little, if at all, from an ordinary secondary school except that the children were practically all boarders.

Although the term orphanage must not be taken to imply that the inmates were necessarily orphans in the strict sense—since in some cases only one parent might be dead, or both might be alive but missing—all the eight schools had this in common, that the children had been sent to them owing to the lack of the usual home advantages enjoyed by other children. At the same time entrance into an orphanage does not cut off the inmates completely from relatives and friends; those who have any are permitted to visit them, if desired, during the annual holiday, which may be a fortnight in the summer or a week at Christmas and another week at some other time. Provision of some kind is made also for those who are without any friends to take an interest in them; some go together to a seaside camp, which is a happy event in their lives. The holidays in school *H* were of the same length as normal school holidays.

Two out of the eight schools visited, *F* and *G*, were quite small, the actual numbers tested in them being only 22 and 25 respectively. In the remaining six schools the number of children tested ranged from about 70 to 220. Four schools, *D*, *E*, *F*, and *G*, contained only boys; the other four contained girls also.

Grading of Social Class.

It was essential for our enquiry that we should have some index of the social origin of each child. The index we used was the occupation of the father, as it was the only one likely to be available, and before proceeding with the investigation in any school we made sure that this information was included in their records.

The occupations were classed under five broad headings, following the well-known principle adopted by the Registrar-General in his discussion of fertility, infant mortality, etc., according to social class (1911).

Class 1 contains professional and clerical occupations. Heads of firms and managers of departments in business are also included here.

Class 3 contains skilled manual workers.

Class 5 contains unskilled labourers.

Class 2 contains such occupations as are intermediate between (1) and (3). It includes, for instance, the great majority of shop assistants.

Class 4 contains such occupations as are intermediate between (3) and (5). It is meant for occupations like that of a tram conductor, who is scarcely a skilled worker and who yet does not rightly go into the same class as a casual labourer.

The occupations for each school in turn were classed under these five heads quite independently by the two of us and our two lists were then compared. There were remarkably few disagreements and after hearing the arguments on both sides there was little difficulty in coming to an agreed decision. If an element of doubt appeared as to the class into which a given occupation should go, the doubt was frequently resolved by considering the kind of company one so engaged would be likely to keep: that after all supplied the touchstone for the purpose of our investigation. While it is not suggested that everyone would reach the same conclusions as ourselves as to where each occupation should go, it is claimed that there is not likely to be a large margin of error, especially if Classes I and II are combined together to form one group, and likewise Classes IV and V to form another, in the final analysis of results.

Testing of Intelligence.

A test was wanted (1) which would serve for boys and girls of the elementary school type, of ages about 9 to 16, and (2) which had established norms—marks standardized—for English children at these ages. All the group-tests obtainable were carefully examined and the Simplex was finally chosen as the one which seemed to satisfy the above conditions best. A more recent test, issued by the Institute of Industrial Psychology, had some equally attractive features, but unfortunately it came to hand too late to receive equal consideration. The Simplex test, moreover, possessed this undoubted advantage over the other: it had been used pretty widely and the results obtained by previous workers would, it was hoped, prove valuable for purposes of comparison.

We desire to thank Mr J. H. Wilson, Brighton, for assistance in the choice of a test and for useful suggestions as to its administration. The Simplex directions were followed implicitly. In this we had the co-operation of Miss James, who gave us the benefit of her experience in the conduct of intelligence tests under Mr Cyril Burt.

III. RESULTS.

The test was tried in the first instance with all the children in a single large orphanage, *A*, which included boys and girls from the age of 8 up to 15. The average marks obtained by each age-group are shown in

Table I. There is a reasonably regular gradation of marks with age, but, out of a total of 223 children, 18 scored less than ten marks. Of these 18, four were under 9 years of age, eight were between 9 and 10, three between 10 and 11, one between 11 and 12, and two between 12 and 13. I.Q.'s based upon such low marks, even if they could be calculated in the ordinary way, would not provide a very reliable index of intelligence. The inference drawn from this preliminary trial was that the Simplex test would be unsuitable for very young children but that it might be regarded as fairly reliable for older children. In all subsequent schools tested, therefore, children under 9½ years of age were excluded.

TABLE I. *Orphanage A. Distribution of children according to average marks obtained at different ages.*

Age	No. of children	Aver. marks	Age	No. of children	Aver. marks
8–	4	All under 10	12–	54	54
9–	18	20	13–	46	67
10–	37	38	14–	19	81
11–	45	51			

On the examination of the results obtained in the first three orphanages tested, *A*, *B*, and *C*, it was discovered that even after choosing 9½ as the minimum age-limit there still remained an appreciable number of children scoring less than ten marks who might vitiate any comparisons attempted. When all children under 10½ years of age are cut out, only nine are left, over this age, who obtained less than ten marks. So small a number could scarcely affect the results much, and, when they are omitted, Tables II and III show the mean I.Q.'s according to length of residence of children and occupation class of fathers.

If the children in schools *A*, *B*, and *C* are split up into groups, according to length of residence, and, these again into other groups, according to social origin, the numbers in each group of ultimate subdivision are too small to be of any value. In Tables II and III, therefore, the division is not carried beyond the first stage, *i.e.* the question of length of residence and the question of social class are treated separately, not combined. Table IV, where both questions are treated together broadly, is given with the warning that the numbers in the individual schools are small.

Table II does not suggest any very definite connection between length of residence and the I.Q.'s unless perhaps there is some slight evidence of at first a rise and then a fall in the I.Q. as residence continues. Tables III and IV indicate a decrease in the I.Q. in passing down the social scale as judged by occupation, but this effect is also only slight and not

TABLE II. *Orphanages A, B, C. Distribution of children showing mean* I.Q.*'s according to length of residence.*
(Omitting nine children who obtained less than ten marks.)

Years of residence	All 3 Schools				School *A*				School *B*				School *C*			
	No. of children		Mean I.Q.		No. of children		Mean I.Q.		No. of children		Mean I.Q.		No. of children		Mean I.Q.	
0–	50	128	99·3	98·7	34	91	100·5	99·6	12	30	94·4	96·5	4	7	103·0	96·3
1–	78		98·3		57		99·0		18		97·9		3		87·3	
2–	76	130	99·6	100·0	52	72	100·0	100·5	19	48	99·2	99·9	5	10	97·0	96·2
3–	54		100·5		20		102·0		29		100·4		5		95·4	
4–	43	112	97·5	96·9	7	13	98·9	98·3	31	59	96·8	96·9	5	40	99·8	96·4
5 and over	69		96·5		6		97·7		28		97·0		35		95·9	
All		370		98·6		176		99·9		137		97·9		57		96·4

TABLE III. *Orphanages A, B, C. Distribution of children showing mean* I.Q.*'s according to occupation class of father.*

Occupation Class	All 3 Schools				School *A*				School *B*				School *C*			
	No. of children		Average I.Q.		No. of children		Average I.Q.		No. of children		Average I.Q.		No. of children		Average I.Q.	
1	54	96	99·3	100·2	33	59	100·1	101·3	16	24	95·9	96·9	5	13	104·2	101·1
2	42		101·4		26		102·8		8		98·9		8		99·1	
3		98		98·3		52		100·2		26		99·5		20		91·9
4	79	176	97·3	97·9	25	65	97·6	98·3	52	87	96·7	97·6	2	24	110·0	97·5
5	97		98·3		40		98·7		35		99·1		22		96·4	
All		370		98·6		176		99·9		137		97·9		57		96·4

without exception. Occupation classes (1) and (2), and likewise (4) and (5), are combined because of the smallness of numbers and the occasional difficulty experienced in separating these grades, (1) from (2), and (4) from (5), with any precision.

It was evidently desirable to add to the data in order to reach more definite conclusions. Accordingly five more schools were tested: the two Industrial Schools, *D* and *F*, and the three Orphanages, *E*, *G*, and *H*. With the exception of *H*, all these contained boys only. The boys in *E*, as already explained, were mostly Poor Law boys and in type they appeared to be very similar to those of *D* and *F*, while *G* may be classed along with *A*, *B*, and *C*. It will be remembered that *H*, however, is in a class by itself and approximates much more to a secondary boarding school in character. Practically all the fathers of the children in this school were graded as belonging to classes (1) or (2), whereas the majority of the fathers of the boys in *D*, *E*, and *F* belonged to classes (3), (4) or (5). In school *F* the only boys tested were those who had been in residence for 3 years or longer.

Taking all the schools together, *A* to *H*, the total number of children tested, of age 9½ and over, was 880. Forty-nine of these children obtained less than ten marks, so that it was impossible to calculate I.Q.'s for them by the ordinary procedure. Also, it was discovered that, whereas among the rest of the children, 430 had been in residence at their respective schools for less than 3 years and 401 had been in residence for 3 years or more, among the 49 children who scored less than ten marks, 46 had been in residence for less than 3 years and three only had been in residence for 3 years or more. Further, there is a striking contrast between the proportions in which the 49 children and the remaining 831 are drawn from different social grades, as the following figures show:

(1)	(2)	(3)	(4)	(5)	Total
208	91	153	149	230	831
1	1	10	16	21	49

These results are some indication that length of residence and social origin both affect such 'intelligence' as is measured by the test used. To what extent that which is measured is pure or innate intelligence and to what extent it is an acquired accomplishment is another question. But at any rate it was manifest that, if the I.Q.'s were to be employed at all in attacking this problem, some estimate would have to be made of the I.Q.'s of the 49 children who could not be dealt with in the ordinary

TABLE IV. *Orphanages A, B, C.* I.Q.'s *of children according to occupation class.*

(*Children over 10½ years of age who had been in residence three years or more.*)

Occupation Class	All 3 Schools: No. of children	All 3 Schools: Average I.Q.	School *A*: No. of children	School *A*: Average I.Q.	School *B*: No. of children	School *B*: Average I.Q.	School *C*: No. of children	School *C*: Average I.Q.
1 2	20 17 } 37	99 100 } 99·5	8	106	16	95	13	101
3	47	99	13	100	21	101	13	93
4 5	38 44 } 82	97 97 } 97	12	97	51	98	19	95
All	166	98·1	33	100·5	88	98·0	45	96·3

TABLE V. *Mean* I.Q.'s *according to years of residence and occupation class.*

(*Children over 9½ years of age.* I.Q.'s *estimated for children with less than ten marks.*)

<table>
<tr><th rowspan="3">School</th><th colspan="10">Years of Residence (0–3)
Occupation Class</th><th colspan="10">Years of Residence (3–<i>n</i>).
Occupation Class.</th></tr>
<tr><th colspan="4">1 and 2</th><th colspan="2">3</th><th colspan="4">4 and 5</th><th colspan="4">1 and 2</th><th colspan="2">3</th><th colspan="4">4 and 5</th></tr>
<tr><th colspan="2">No.</th><th colspan="2">I.Q.</th><th>No.</th><th>I.Q.</th><th colspan="2">No.</th><th colspan="2">I.Q.</th><th colspan="2">No.</th><th colspan="2">I.Q.</th><th>No.</th><th>I.Q.</th><th colspan="2">No.</th><th colspan="2">I.Q.</th></tr>
<tr><td><i>A</i></td><td colspan="2">—</td><td colspan="2">—</td><td>48</td><td>99·1</td><td colspan="2">67</td><td colspan="2">96·6</td><td colspan="2">—</td><td colspan="2">—</td><td>13</td><td>100·1</td><td colspan="2">12</td><td colspan="2">97·2</td></tr>
<tr><td><i>B</i></td><td colspan="2">10</td><td colspan="2">104·6</td><td>—</td><td>—</td><td colspan="2">58</td><td colspan="2">96·6</td><td colspan="2">17</td><td colspan="2">94·9</td><td>—</td><td>—</td><td colspan="2">53</td><td colspan="2">96·9</td></tr>
<tr><td><i>D</i></td><td colspan="2">—</td><td colspan="2">—</td><td>—</td><td>—</td><td colspan="2">52</td><td colspan="2">81·2</td><td colspan="2">—</td><td colspan="2">—</td><td>—</td><td>—</td><td colspan="2">15</td><td colspan="2">80·5</td></tr>
<tr><td><i>E</i></td><td colspan="2">—</td><td colspan="2">—</td><td>—</td><td>—</td><td colspan="2">42</td><td colspan="2">95·0</td><td colspan="2">—</td><td colspan="2">—</td><td>—</td><td>—</td><td colspan="2">46</td><td colspan="2">97·1</td></tr>
<tr><td></td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td><td>No.</td><td>I.Q.</td></tr>
<tr><td><i>H</i></td><td>45</td><td>113·0</td><td>14</td><td>114·0</td><td>—</td><td>—</td><td>—</td><td>—</td><td>—</td><td>—</td><td>93</td><td>108·8</td><td>26</td><td>108·1</td><td>—</td><td>—</td><td>—</td><td>—</td><td>—</td><td>—</td></tr>
<tr><td rowspan="2">All*</td><td>92</td><td>107·3
107·1</td><td>46</td><td>104·4
103·9</td><td>98</td><td>96·3
94·3</td><td>91</td><td>95·5
92·4</td><td>149</td><td>93·2
90·6</td><td>117</td><td>106·0</td><td>46</td><td>105·6</td><td>65</td><td>95 5</td><td>74</td><td>96·0
95·4</td><td>102</td><td>93·7
93·5</td></tr>
<tr><td colspan="2">138</td><td colspan="2">106·4
106·0</td><td></td><td></td><td colspan="2">240</td><td colspan="2">94·1
91·2</td><td colspan="2">163</td><td colspan="2">105 9</td><td></td><td></td><td colspan="2">176</td><td colspan="2">94·7
97·3</td></tr>
</table>

* Of the bracketed I.Q.'s the upper in each case is the result obtained when the upper limit is taken in the estimate of I.Q.'s of children who got less than ten marks in the test, and the lower is the result when the lower limit is taken.

way. To ignore them would certainly in some degree prejudice the result.

An estimate was made on the following plan. Had each of the 49 children obtained exactly ten marks their I.Q.'s would have ranged from about 60 to 90, according to age. This gives for them an upper limit. Now very few children among those who scored ten marks or over had I.Q.'s below 70, although two or three fell as low as 61. Terman classified an I.Q. below 70 as indicative of definite feeble-mindedness, but his criterion clearly cannot apply to children like many of those tested in the present instance who have been considerably handicapped in various ways during the early years of life. It would not seem unreasonable to assume, quite tentatively, as a lower limit for the I.Q.'s of the 49 weaklings a range of something like 45 to 70, or three-quarters of their upper limits. Adopting this principle the mean I.Q.'s of all the combined schools are shown in Table V, according to length of residence of the children and occupation class of the fathers. I.Q.'s are only recorded for individual schools where the number of observations on which the means are based is as high as ten. Also, in calculating these I.Q.'s for individual schools, instead of finding an upper and a lower limit as explained above, an in-between value equal to nine-tenths of the I.Q. (according to age) corresponding to ten marks was used.

The differentiation of intelligence according to class comes out more distinctly in this table, when all the schools are combined, and it is present whether the length of residence has been relatively long or short. Also, there are slight but pretty consistent indications of a closing up of the I.Q.'s in children of 3 years' residence and over when compared with the shorter period, as if the brighter children became less bright while the duller children became less dull during their school career. It is possible that the attempt to fit many children into one mould may curb the imagination and initiative of the more intelligent, and it is also possible that the brighter children more readily get jobs so that those who are left are a selected and not a random lot of children. This second possibility will be examined presently. Again, children who appear to be very dull during their early days at school may be suffering from a failure to read rather than from a failure to understand in the true sense; and, as their reading improves, so their intelligence appears to expand. The Simplex, like all other tests, puts some premium on school acquirements. The differences noticed must not, however, be pushed too far, because their probable errors are large (see Table VI) and these may therefore rob them of all significance.

TABLE VI. *Difference in mean* I.Q. *according to length of residence.*

I.Q. greater in earlier period of residence			I.Q. greater in later period of residence.		
School	Occupation Class	Difference in mean I.Q.	School	Occupation Class	Difference in mean I.Q.
B	1 and 2	9·7 ± 3·7	*A*	3	1·0 ± 2·4
H	1	4·3 ± 1·8	*A*	4 and 5	0·6 ± 1·7
H	2	5·9 ± 3·0	*B*	4 and 5	0·3 ± 1·7
D	4 and 5	0·7 ± 2·5	*E*	4 and 5	2·1 ± 1·8

In order to find out whether the apparent depreciation of intelligence with residence in the brighter children could be explained as due merely to a selective influence which took the brighter children away from school first, the same calculations were repeated omitting all children of 14 years of age and over. The results are shown in Table VII. It will be seen that for the combined schools and when we unite classes (1) and (2), the effect is still there, if anything, more pronounced, although it is rather less pronounced in school *H*. The increase in I.Q. of the children in the lower occupation classes, (3), (4), and (5), is also still apparent.

TABLE VII. *Mean* I.Q.*'s according to years of residence and occupation class.* (*Children between* 9½ *and* 14 *years of age.* I.Q.*'s estimated for children with less than ten marks.*)

	Years of Residence (0–3)						Years of Residence (3–*n*)					
	Occupation Class						Occupation Class					
	1 and 2		3		4 and 5		1 and 2		3		4 and 5	
School	No.	I.Q.	No.	I.Q.	No.	I.Q.	No.	I.Q.	No.	I.Q.	No.	I.Q.
A	54	101·5	47	100·5	66	97·5	6	—	8	—	9	99
B	9	105·5	7	—	57	96	15	94·5	17	102	48	97
C	—	—	9	91	11	95	11	101	8	—	16	95·5
D	4	—	12	87·5	36	84	—	—	2	—	6	—
E	1	—	10	96·5	41	95	3	—	4	—	43	96
F	—	—	—	—	—	—	—	—	2	—	9	92·5
G	—	—	4	—	10	98	—	—	2	—	2	—
H	48	115·5	1	—	—	—	50	112	—	—	3	—
All	116	107 5	90	97	221	94·5	85	106·5	43	98·5	136	96·5

	Occupation Class					Occupation Class				
All	1	2	3	4	5	1	2	3	4	5
Schools	108·5	105	97	94 5	94 5	107	106	98·5	97	95·5
	107·5			94 5		106 5			96·5	

It was felt that these results might be criticized on two grounds: (i) the norms upon which the I.Q.'s are based, though possibly applicable to ordinary elementary schools, may not be appropriate for orphan and industrial schools; and, in particular, the estimated I.Q.'s for children who scored less than ten marks may not be satisfactory; (ii) by combining schools which are not really of a type the means obtained may be purely artificial and have little relation to the real facts, with the result that the issue may only have been confused. It seemed therefore worth while to make a fresh analysis of all the data in an entirely different way, using crude marks instead of calculated I.Q.'s.

With this object all the children who took the test and who were between the ages of 9½ and 14 were divided into four groups according to age, 9½–11, 11–12, 12–13, 13–14. Any possibility of selection affecting the results, through the speedier placing of the brighter children in jobs, was obviated by the omission of children over 14. Even a later age would have sufficed in one or two schools where all the children are normally kept on until they are 15 or 16.

The median and quartile marks were determined for each group and these provided standards by reference to which the performance of children in smaller groups and in single schools could be judged. It would have been equally possible to work with arithmetic means and standard deviations instead of with medians and quartiles, but the latter were chosen because they are (1) more readily determined, (2) more easily understood, and (3) not distorted by the undue influence of exceptional children. These standards are shown in Table VIII.

TABLE VIII. *Quartile and median marks according to age.*
(*All schools. All occupation classes.*)

Rank	Age (9½–11)	Age (11–12)	Age (12–13)	Age (13–14)
Upper Quartile	47	65	71	86
Median	28·5	40·5	53	67
Lower Quartile	15	31	37	49·5
No. of Observations	152	178	172	189

The next table deals with 'all children'; they are dissected in such a way as to bring out the effect of residence and the effect of class. It is arranged on a percentage basis, so that the sum of related percentages in each vertical column is 100 to within a unit in the first decimal place: Column (14) is inserted merely as a reminder that all classes and all years are united to form the measuring-gauge. Columns (12) and (13) indicate that performance in the tests, for all classes combined, is improved by residence, the percentage above the median rising and the percentage below falling, and this seems to be true generally in each occupation class as shown by Cols. (2) to (11). It would be more satisfactory of course if the *same* children could be tested before and after 3 years' residence,

TABLE IX. *Percentage distribution of children in all schools according to occupation class and according to years of residence relative to* TABLE VIII *standards.*

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Years of Residence (0–3) Occupation Class					Years of Residence (3–*n*) Occupation Class					All Classes Years of Res.		All Classes All years
Rank	1	2	3	4	5	1	2	3	4	5	(0–3)	(3–*n*)	
Above the Upper Quartile	45·0	43·1	16·1	12·5	15·4	45·8	38·5	26·7	21·8	19·6	22·8	29·0	25
	44·4			14·3		43·5			20·6				
Between the Median and U.Q.	26·9	11·1	28·3	20·5	23·4	35·6	38·5	23·3	22·6	21·6	23·4	26·9	25
	22·0			22·2		36·5			22·1				
Between the Median and L.Q.	19·4	26·4	25·0	34·1	27·4	8·5	11·5	24 4	32·3	28·4	26·7	22·5	25
	21·6			30·1		9·4			30·1				
Below the L.Q.	8·7	19·4	30·6	33 0	33·8	10·2	11·5	25·6	23·4	30·4	27·1	21·6	25
	12·1			33·5		10·6			27·2				
Above the Median	71·9	54·2	44·4	33·0	38·8	81·4	77·0	50·0	44·4	41·2	46·2	55·9	50
	66·4			36·5		80·0			42·3				
Below the Median	28·1	45·8	55·6	67·1	61·2	18·7	23·0	50·0	55·7	58·8	53·8	44·1	50
	33·7			63·6		20·0			57·3				
No. of Observations	80	36	90	88	133	59	26	43	62	74	427	264	691

instead of different children, as here, who may not as groups be equally endowed with intelligence.

Again, looking at Cols. (2) to (6), there is a marked difference between the proportions above and below the median in classes (1) and (2) as compared with classes (3), (4), and (5). Taking classes (1) and (2) together, for years of residence (0–3), the numbers above and below the median are as 2 is to 1, whereas for classes (4) and (5) combined, this proportion is almost exactly reversed. Similar differences are observable among the children of different classes who have been in residence for 3 years and over: the proportion then becomes 4 to 1 for classes (1) and (2), and about 2 to 3 for classes (4) and (5).

It was pointed out that among the various schools tested, one, namely *H*, was of a distinctly superior type. As it contained 102 children between the ages of 9½ and 14, and 98 of them were in classes (1) or (2), it may be suggested that their inclusion is likely to bias the standardization of marks (*i.e.* the fixing of the median and quartile points) in the four age-groups in such a way as to affect the final result. Accordingly, a new calculation was made, omitting this school, and the new standards are shown in Table X. The next table shows how the four schools, *A*, *B*, *C*, and *G* combined, come out on this standard. These schools contained children who were very much of a type and it is believed that they may be safely combined. Schools *D*, *E*, and *F* comprising children of a slightly inferior type may be combined in like manner (see Table XII). Without going into details it may be stated that these tables broadly confirm the results previously obtained, except that for classes (1) and (2) together the percentage above the median does not increase with length of residence. There is in fact an increase in the percentage above the upper quartile but it is more than balanced by a decrease in the percentage between the median and the upper quartile.

TABLE X. *Quartile and median marks according to age.*

(*All schools except H. All occupation classes.*)

Rank	Age (9½–11)	Age (11–12)	Age (12–13)	Age (13–14)
Upper Quartile	41	63	66	80
Median	24	44	51	60
Lower Quartile	10	28	36	47
No. of Observations	131	151	152	155

So far stress has been laid on the average I.Q. and the average marks of various groups and the effect which length of residence or social origin has upon them. It is of interest also to examine the variability of intelligence within any group. For instance, the effect of residence upon

TABLE XI. *Percentage distribution of children in certain schools according to occupation class and years of residence relative to* TABLE X *standards.*

(*Four Orphan Schools: A, B, C, G.*)

	Years of Residence (0–3) Occupation Class					Years of Residence (3–*n*) Occupation Class					All Classes Years of Res.		All Classes All years
Rank	1	2	3	4	5	1	2	3	4	5	(0–3)	(3–*n*)	
Above the Upper Quartile	23·8	39·1	26·1	18·1	24·0	31·2	40 6	40·0	25·6	25·0	24·3	31·3	26·4
	29·5			21·2		35·9			25·3				
Between the Median and U.Q.	43·8	17·4	28·4	31·2	26·7	31·2	21·9	17·1	20·7	39·7	29·9	25·7	28·2
	34·1			28·8		26 6			29·3				
Between the Median and L.Q.	21·2	23·9	19·4	21·0	29·3	21·9	18·8	21·4	34·1	16·2	23·2	23·6	23·1
	22·2			25·3		20·3			26·0				
Below the L.Q.	11·2	19·6	26·1	29·7	20·0	15·6	18·8	21·4	19·5	19·1	22·6	19·4	22·3
	14·6			24·7		17·2			19·3				
Above the Median	67·6	56·5	54·5	49·3	50·7	62·4	62·5	57·1	46·3	64·7	54·2	57·0	54·6
	63·6			50·0		62·5			54·6				
Below the Median	32·4	43·5	45·5	50·7	49·3	37·5	37·6	42·8	53·6	35·3	45·8	43·0	45 4
	36·5			50·0		37·5			45·3				
No. of Observations	40	23	67	69	75	16	16	35	41	34	274	142	416

TABLE XII. *Percentage distribution of children in certain schools according to occupation class and years of residence relative to* TABLE X *standards. (Two Industrial Schools and one Poor Law School: D, E, F.)*

Rank	Years of Residence (0–3) Occupation Class			Years of Residence (3–*n*) Occupation Class			All Classes Years of Residence		All Classes All years
	3	4	5	3	4	5	(0–3)	(3–)	
Above the Upper Quartile	18·2	15·8	17·2	(12·5)*	26·3	17·9	18·3	18·8	18·9
		16·9			20·7				
Between the Med. and U.Q.	22·7	0·0	19·0	(25·0)*	26·3	17·9	15·4	24·6	19·5
		14·3			20·7				
Between the Med. and L.Q	25·0	52·6	18·1	(37·5)*	26·3	28·2	26·9	27·5	27·8
		26·6			27·6				
Below the L.Q.	34·1	31·6	45·7	(250)*	21·1	35·9	39·4	29·0	33·7
		42·2			31·0				
Above the Median	40·9	15·8	36·2	(37·5)*	52·6	35·8	33·7	43·4	38·4
		31·2			41·4				
Below the Median	59·1	84·2	63·8	(62·5)*	47·4	64·1	66·3	56·5	61·5
		68·8			58·6				
No. of Observations	22	19	58	(8)*	19	39	104	69	173

* The observations here are too few to be of any value.

such variability may be tested by calculating the standard deviation of the I.Q.'s of the individuals comprising any group, for a shorter period of residence (0 to 3 years) and for a longer period (3 years and over), and finding whether the difference between the two results exceeds its probable error (see Table XIII). The coefficient of variation has also been calculated and is shown in the last two columns of the table. Another plan is to combine like schools and to deal with a single age-group at a time, comparing now the variability in marks for a shorter and a longer period of residence (see Table XIV). Both methods, with one or two

TABLE XIII. *Relation of variability of* I.Q.*'s to length of residence.* (*Different classes in various schools tested by Standard Deviation.*)

School	Occupation Class	No. of Observations (0–3)	(3–*n*)	Yrs. of Residence (0–3) S. D.	(3–*n*) S. D.	Decrease in S.D. with increase of Residence	Yrs. of Residence (0–3) Coef. of var.	(3–*n*)
A	3	48	13	15·4	10·3	5·1 ± 1·7	15·5	10·3
A	4, 5	67	12	14·8	6·6	8·2 ± 1·2	15·3	6·8
D	4, 5	52	15	14·5	12·2	2·3 ± 1·8	17·8	15·2
E	4, 5	42	46	13·4	12·4	1·0 ± 1·3	14·1	12·8
H	1	45	93	15·4	11·6	3·8 ± 1·2	13·6	10·8
H	2	14	26	14·6	12·2	2·4 ± 2·2	12·8	11·3
B	1, 2	10	17	16·1	8·9	7·2 ± 2·6	15·4	9·4
B	4, 5	58	53	13·0	13·3	–0·3 ± 1·2	13·5	13·7

TABLE XIV. *Relation of variability of marks to length of residence.* (*Different age-groups in various schools tested by Standard Deviation.*)

Schools	Age-Group	No. of Observations (0–3)	(3–*n*)	Yrs. of Residence (0–3) S. D.	(3–*n*) S. D.	Change in S.D. with increase of Residence Decrease	Increase	Yrs. of Res. (0–3) C.V. %	(3–*n*) C.V. %
A, B, C, G	9½–11	94	14	21·0	20·0	1·0	—	70	77
,,	11 –12	73	28	22·8	29·8	—	7·0	47	56
,,	12 –13	64	48	20·9	20·9	—	—	42	41
,,	13 –14	43	52	23·8	21·7	2·1	—	37	32
D, E	9½–11	19	1	15·5	—	—	—	75	—
,,	11 –12	29	20	25·1	20·1	5·0	—	63	48
,,	12 –13	27	10	26·7	12·6	14·1	—	66	27
,,	13 –14	29	27	24·2	27·2	—	3·2	48	45
H	9½–11	12	9	23·7	16·4	7·3	—	40	28
,,	11 –12	16	11	24·4	16·0	8·4	—	28	21
,,	12 –13	8	12	31·2	20·6	10·6	—	37	27
,,	13 –14	21	13	25·5	27·3	—	1·8	29	25

exceptions, point to a diminution in variability as the period of residence extends, which is in line with a tentative conclusion previously reached. Further, the last two columns of Table XIV suggest that there may also be diminution in variability of intelligence as the age increases, though irregularities appear where the number of observations is small.

Another subject of interest is change in intelligence with age. This was tested first by finding the arithmetic mean of the marks of the children in each school in successive age-groups, thence determining the average mental age and a mean I.Q. for each age-group in each school (see Table XV). Secondly, it was tested by finding the median marks of certain combined schools in the same age-groups, again deducing the average mental age and a mean I.Q. for each group (see Table XVI). With slight exceptions both tables bear witness to the same general tendency; at first an improvement in the I.Q., followed by a relapse after the age of 12.

One further test was made, simply as a check on the last results. The children of ages 10–, 11–, 12–, 13–14 in *A*, the largest school examined, were considered separately. Working with the individual I.Q.'s, the mean and the variability for each age-group were determined, with the following result:

Age-group	10–	11–	12–	13–
No. in group	37	45	54	46
A.M. of I.Q's	104 ± 23	102 ± 29	96 ± 12	96 ± 13
S.D. of I.Q's	14·5 ± 16	16·9 ± 20	11·4 ± 8	11·4 ± 9
Coef. of variation	14 %	17 %	12 %	12 %

This table brings out the fact, frequently commented upon by other observers, that the amount of variation in intelligence within such groups as these is very large, far larger than the difference between the means of the several groups would lead one to suspect. It follows that, since

TABLE XV. *Change in* I.Q. *with age.* (*First method. Based on a determination of* A.M. *marks.*)

	I.Q.'s at ages as below											
	9½–11		11–12		12–13		13–14		14–15		15–16	
School	No of Obs.	I.Q.	No. of Obs.	I.Q	No. of Obs.	I.Q	No. of Obs.	I.Q.	No. of Obs.	I.Q.	No. of Obs.	I.Q.
A	55	103	45	104	54	98	46	98	19	97	—	—
B	45	96	36	100	39	100	33	98	15	93	—	—
C	14	100	15	107	17	90	9	(89)	15	89	—	—
D	7	(84)	20	93	16	86	17	80	24	80	11	76
E	13	101	29	99	21	92	39	96	6	(95)	—	—
H	21	122	27	116	20	111	34	110	—	—	—	—

[Where the I.Q. is bracketed the number of observations is below 10.]

TABLE XVI. *Change in* I.Q. *with age.* (*Second method. Based on a determination of median marks.*)

Group	I.Q.'s at ages as below 9½–11	11–12	12–13	13–14
All Schools	99	100	97	97·5
All Schools except *H*	96	98·5	95	93
Schools *A*, *B*, *C*, *G*				
(i) Years of Residence (0–3)	98	100	97	94
(ii) Years of Residence (3–)	93	105	99	97·5
Schools *D*, *E*				
(i) Years of Residence (0–3)	92	94	87	89
(ii) Years of Residence (3–)	–	94	95	92

the number of observations is small, the observed differences between the means and variabilities at different ages might easily be due simply to random sampling. If they have any significance at all they again point to a decrease in intelligence and in variability of intelligence as age advances. Since a normal group of children, normal that is according to the standard of the Simplex scale of marks, should retain a mean I.Q. level of 100 at each age, it is clear that, if the fall in intelligence with the rise in age be significant, it must be associated in some way with residence at an institution and the conditions of life experienced there, because increase in age and in period of residence in general go together.

IV. DISCUSSION.

The cumulative impression produced by a study of the investigations quoted at the outset of this paper is impressive. Whenever the problem has been attacked an association has been found between good performance in intelligence tests and good social origin. The question next to be asked is whether this result can be accounted for by differing out-of-school environment. It may be at once remarked that certain evidence from such investigations as those to which reference has been made is relevant. Very great variability in intelligence within any group of similar social origin is always observed. This observation is presumably to be interpreted as indicating that similar out-of-school environment does little towards producing similarity in performance. A presumption is thus raised in favour of the view that different out-of-school surroundings are not likely to produce considerable differences in performance. It may further be remarked that different out-of-school surroundings, if effective, might be so in either or both of two different ways. It might be that the tests would favour children from good surroundings as, for instance, by giving them better equipment in linguistic and other attainments, or it might be that they would encourage mental growth much as growth in stature can to some extent be promoted by favourable surroundings. Nearly all intelligence tests do, it is agreed, put some premium on linguistic attainments but against that as a complete explanation of the differences observed is to be set the fact that non-linguistic tests and teachers' estimates of intelligence, which are intended to discount home and school acquirements, are both found in general to be markedly correlated with the results reached by the use of intelligence tests. Further, in the case of orphanage children, even when they have not been many years in an institution, differences in linguistic and other attainments derived from differences in former home surroundings are not likely to form serious disturbing factors, because among children growing up together for a few years these traces of various previous experiences are likely soon to be lost. It would thus seem that if out-of-school environment is effective at all it can only be so by encouraging or inhibiting the development of innate gifts.

The ideal material for the solution of the problem would be found among children of different social origin brought up in the same institution from a very early age. Can it be said that our material, defective as it is, aids us towards a solution? Tables V and XI show that when children of 3 years' residence and over are graded according to the occupation of the fathers there is evidence of an association between social origin and intelligence by whichever method the data are treated. There is no means of deciding whether these differences in intelligence between children of different social grades are as great as between children not brought up in institutions, because there is no standard grading of intelligence with class. But it may be worth noting that the differences found are not dissimilar in degree to those found in the Northumberland investigation. There is at least no definite suggestion that this length of residence obscures initial differences so far as these tables can throw light upon the matter.

Some further progress towards a solution is afforded by other observations. Indications are offered that environment is not without effect. These indications consist in the facts (*a*) that the I.Q. tends to increase among the younger of these children (*i.e.* generally, the children who have been resident a short time) and to decrease among the older (*i.e.* generally, the children who have been in residence a longer time), (*b*) that intelligence is apparently positively associated with length of residence in the case of children from the lower occupation classes and negatively associated with length of residence in the case of children from the upper social classes, and (*c*) that, in part the result of this contraction at either end, variability of intelligence apparently decreases within groups of similar origin as length of residence increases. The phenomenon recorded under (*a*) is noteworthy because both Schmitt and Gordon working with orphanage children made somewhat similar observations. Whether any importance attaches here to (*a*) depends upon the confidence reposed in the Simplex standards. If the confidence is well placed then the institutional environment stimulates intelligence at first and somewhat inhibits it later, presumably because it is better than that experienced previously by these children at home but not so stimulating as that usually experienced in later years in the ordinary elementary schools outside. The phenomena recorded under (*b*) and (*c*), if real, and it must be remembered that we are dealing merely with indications, are susceptible only of explanation through action of the environment. The indication, however, is that the action is no more than slight. There is no reason to suppose, so far as this evidence is concerned, that environmental influences are the whole or even the major part of the cause of the differences in intelligence between children of different social origin.

This conclusion is only what might be anticipated on quite other grounds. If men are to some degree occupationally selected so that the more intelligent obtain on the whole the better positions and if intelligence is also inherited, we should expect to find a rather higher level of intelligence among the children of parents of higher social status where social status is defined by occupation.

V. CONCLUSIONS.

(i) There appears to be a differentiation of 'intelligence' according to class, whether intelligence is judged by the I.Q.'s or by the proportion of children who obtained more than average marks in each class; (ii) this effect is present among children who have been in residence less than 3 years and among those who have been in residence 3 years and over; (iii) children in occupation classes (3), (4), and (5) who have been in residence for 3 years and over tend to have higher I.Q.'s than those who have been in residence for less than 3 years. Also, a higher percentage get marks above the average among those who have been longer in residence in those classes; (iv) this effect is certainly less marked, if not in some cases reversed, for children in occupation classes (1) and (2); (v) there appears to be a diminution in variability of intelligence within like groups judged by I.Q.'s and by marks, as the period of residence extends and also possibly as the age increases; (vi) the mean I.Q. tends to increase between the ages 9½ and 12, but afterwards it tends to decrease.

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CRITICAL NOTICE

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The Measurement of Intelligence. By E. L. THORNDIKE, E. O. BREGMAN, M. V. COBB, ELLA WOODYARD and the STAFF of the DIVISION of PSYCHOLOGY of the Institute of Educational Research of Teachers' College, Columbia University. New York: Columbia University, 1927. Pp. xxvi + 616.

An event has happened which no psychologist can afford to overlook. More than four years ago a leader—many would say *the* leader—in modern psychology, Prof. Edward Thorndike, accepted a grant from the Carnegie Corporation for the purpose of undertaking a research of unusual magnitude. To this he proceeded to devote, it would appear, not only all his own indefatigable activity—laying aside even his work as university teacher—but also the energies of his entire staff. And now at last, after these years of waiting, we are presented with the outcome of such great and auspicious labour. It is incarnated in a large volume having as title, *The Measurement of Intelligence.*

The account sets out with a frank recognition that hitherto the testing of intelligence, despite its having attained to enormous dimensions and popularity, nevertheless really remains in a very unsatisfactory state. The current tests are said by the authors to suffer from "three fundamental defects"; "just what they measure is not known"; "just how far it is proper to add, subtract, multiply, divide, and compute ratios with the measures obtained is not known"; and "just what the measures obtained signify concerning intellect is not known."

Such a condition of affairs is it, then, that the present investigation has undertaken to remedy. The general plan adopted is to conceive the intellect as possessing an 'area' with the two dimensions of 'altitude' and 'width.' By the former term is meant "the degree of difficulty at which a person can succeed with tasks"; whereas the 'width' indicates "the number of tasks that he can succeed with at any specified degree of difficulty." In accordance with this plan, the investigation has aimed at devising and executing methods whereby degrees of difficulty can be submitted to exact estimation. For such purpose, the authors have brilliantly combined two cardinal propositions, each in itself familiar enough. The first is the obviously reasonable one, that the difficulty of an operation may be taken as inversely proportional to the frequency of persons succeeding at it. His second basal proposition—one, however, by no means undisputed—asserts that the frequency distribution of degrees of ability, for the classes of person likely to be at issue, fall into the so-called 'normal' curve. If, then, he argues, the base line of this frequency curve be divided into parts of equal length, such a length will supply a constant unit for comparing together differences, not only in degree of ability, but also in that of difficulty, throughout the whole scale. Such a theorem in mental measurement—with all its assumptions, its possible pitfalls both statistical and psychological—may well supply matter to examine and discuss for many a day.

But the concepts of 'altitude,' 'width' and 'difficulty' can have little meaning and no utility until some notion is forthcoming as to the field to which they are going to be applied. And this field, as we have seen, and as the authors emphatically reiterate, consists in the 'intellect.' Here, then, we arrive at the chief of the 'fundamental defects' which are admitted to have hitherto obstructed psychology and which are now to be remedied. We are faced once more, that is to say, by the same old question on which all previous attempts to measure ability have foundered. We are compelled to ask what sort of thing this 'intellect' is intended to denote. On first impression, it sounds like nothing so much as one of the ancient general faculties, which modern psychologists commonly take their chief pride in having at last outgrown.

When the reader of the volume looks for a reply to this great question, he must be prepared for some passing shocks. Such general faculties do actually seem to have found in Thorndike—of all people!—a new champion. He writes:

> Presumably a man can use intellect and display the amount of it which he possesses in operations with any sort of material object, any living plant or animal, including himself, any quality or relation that exists in reality or in imagination, any idea or emotion or act.

All this flows from the same pen, it will be remembered, by which, in 1903, every kind whatsoever of general faculty was categorically banned in terms that have since become classical:

> The science of education should at once rid itself of its conception of the mind as a sort of machine, different parts of which sense, perceive, discriminate, imagine, remember, conceive, associate, reason about, desire, choose, form habits, attend to.... There are only the particular connections between particular mental events and others....The mind is a host of highly particularized and independent faculties[1].

[1] *Educational Psychology*, 1st ed.

When we seek at least to be told what sort of operations fall within the purview of this general intellect, the first response is again startling. The definition which is to set us on the way towards remedying the 'fundamental defect' of current psychology runs as follows:

> For a first approximation, let intellect be defined as that quality of mind (or brain or behaviour if one prefers) in respect to which Aristotle, Plato, Thucydides, and the like, differed most from Athenian idiots of their day.

We begin to wonder whether the author is taking us seriously.

Surely, these Athenian idiots furnish a singularly inaccessible basis of investigation. And how do we even know that all of them differ most from Aristotle 'and the like' in any one thing? May not some differ more in some things and others in others?

Nor does the situation appear to be much cleared up by the psychological considerations afforded. For example, the author argues that the range of the intellect cannot possibly be restricted to the educing of relations and of correlates, since it indubitably also includes 'paragraph reading,' 'information,' 'vocabulary,' as, too, the processes of 'analysis,' 'selection,' and 'organization.' Surely, he cannot think that these six achievements are, or ever can be, effected *without* relational cognitive processes!

Later in the book, however, we do arrive at a perfectly definite solution of the equivocality, and therewith we may see a prospect of obtaining a really workable basis. It is, that every psychologist has the right to understand by 'intellect' whatever he pleases.

> What abilities and tasks shall be treated as intellectual is essentially a matter of arbitrary assumption or choice at the outset.... After the first choice is made, tasks not included in it, and even not known, may be found to correlate perfectly with the adopted total, and so to be 'intellectual.'

Thorndike and his collaborators, for their 'first choice,' assign the title of intellect to the four lines of ability specified below.

C. To supply words so as to make a statement true and sensible.
A. To solve arithmetical problems.
V. To understand single words.
D. To understand connected discourse as in oral directions or paragraph reading.

Intellect as so defined they formally name 'intellect CAVD.' And upon this 'intellect CAVD' it is that, actually, almost the whole of their laborious investigation has been carried out. It is in respect of this that they have eventually arrived at their main results.

Paramount among these stands one that would seem to have been dominantly influential in Thorndike's remarkable increase of favour towards general faculties. For although he continues to accept such faculties in large numbers; although he even declares "it should be unquestioned" that "we may usefully entertain the abstract conceptions" of hearing ability in general, discrimination in general, motor skill in general, and so forth *ad lib.*; still in the end he unexpectedly concedes that any such 'total ability' may sometimes on investigation turn out to be nothing more than "just a name for some concatenation of aspects which various fundamental abilities take under rather artificial conditions." So far as his own present work goes, however, this has decidedly *not* been the case. On the contrary—and herein would appear to lie the crown of the whole investigation—his 'intellect CAVD' has eventually proved to represent what he calls "some unified, coherent, fundamental fact in the world."

Of what nature, then, we hasten to inquire, is the evidence upon which he has succeeded in basing such a momentous conclusion? It consists, we find, essentially, and even solely, in showing that the different 'altitudes' in this 'intellect CAVD' correlate with one another to nearly or quite as great an amount as their attenuation by random influences will permit; in other words, the correlations between the different altitudes reveal values which, on being corrected for attenuation, approach perfect unity. And of just the same nature, as we have already seen (in the second quotation on page 367), is also the evidence upon which the authors propose to base themselves when they wish to include in their 'intellect' any further tasks that did not enter into the originally chosen CAVD.

Now, this line of evidence no one is less likely to impugn than myself. For it happens to be no other than the very proof which I first employed to demonstrate the existence of 'g[1].' Furthermore, every other proof of 'g' advanced by our school ever since (save only the 'inter-columnar correlation,' which was avowedly a provisional substitute) has been just the same thing, merely put into a more convenient mathematical form. In particular, this proof by way of perfect correlation as corrected for attenuation is mathematically identical with the proof now being widely employed by way of 'tetrad differences[2].' If and when Thorndike follows up his own declared programme, and examines what further tasks not comprised in his 'first choice' of the 'intellect CAVD' may nevertheless—on suitably correcting for attenuation[3]—show an approximately perfect correlation with it; if and when he does this, he will surely find, as we have done from the very beginning[1], that this same "unified, coherent, fundamental fact in the world" extends throughout all cognitive abilities whatever. It is neither more nor less than just our '*g*' itself.

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[1] *Amer. J. Psych.* 1904, xv, pp. 268–272.

[2] To substantiate this statement, reference must be made to a book of mine just published, *The Abilities of Man* (Macmillan).

[3] By "suitably" is meant in just the same manner as described in the above quoted pages of the *Amer. J. Psych.*

This pleasing step towards reconciliation between his school and ours is not the sole one. It supplies, rather, the last of a series. In 1903, as the quotation at the bottom of page 366 shows, he still held the view that all abilities are highly particularized and independent. Binet, on the other hand, asserted that they are bundled up into large general faculties. But we for our part introduced the analysis into a single general factor and innumerable specific ones. Explicitly on the strength of this analysis, we advocated the procedure of throwing a most heterogeneous assortment of tests into a single pool, so that the specific factors should tend to cancel one another, leaving behind a good measure of the general factor alone[1]. Neither Binet nor Thorndike accepted this view. But they *did forthwith adopt the corollary of heterogeneous pooling*; although, as we may venture to submit, this had no justification in their own doctrines[2]. Later on, a second corollary from our view was also almost universally adopted; this was that of choosing as the tests to be pooled those which—in spite of being unlike in appearance—nevertheless exhibited high correlations with one another, thereby showing the general factor to be influential. Yet a third great move towards concord was made when the custom arose of radically distinguishing 'general' from 'specific' or 'special' abilities —thereby adopting the very words coined by us; in vain one may search for anyone else using them in or soon before the year 1904. And now finally, with the present volume, Thorndike and his school have taken the only large step yet left towards bringing both sides together —to the joy of the friends of psychology as a positive science. It is a great achievement, and from such a source we could expect nothing else.

[1] *Amer. J. Psych.* 1904, xv, pp. 268–272.

[2] As regards Binet, this is at once obvious from his writing as follows: "The mental faculties of each subject are independent and unequal; with a little memory there may be associated much judgment....Our mental tests, always special in their scope, are each appropriate to the analysis of a single faculty" (*Les Idées sur les Enfants*, 1909). Clearly, then, he ought for each year of his scale to have measured each of these faculties separately, instead of pooling them all together.

As regards Thorndike, reference for the proof must be made to the work quoted in the foot-note to page 366.

CRITICAL NOTICE

BY BRENDA Z. SELIGMAN.

Psychology and Ethnology. By W. H. R. RIVERS. Edited with Preface and Introduction by G. ELLIOT SMITH. (London: Kegan Paul, Trench, Trubner and Co. Ltd., 1926.) Pp. xxviii + 324. 10*s.* net.

A clear statement of Rivers' attitude towards the two sciences to which he devoted his life is seen in a sentence in the first of the studies in this book: "To me, as to most students of the subject, the final aim of the study of society is the explanation of social behaviour in terms of psychology." But social behaviour is a complex process, and adequate explanation cannot be found merely by reference to instincts and emotions. "The business of sociology," he said, is "to ascertain what happens and what has happened before it tries to explain why it happens and has happened" (p. 7). The volume before us does not deal with Rivers' contribution to the exact method of recording "what happens," for which ethnology will always remain in his debt. His concern with "what has happened" arose during his researches into Melanesian Society, when he detected many different types of behaviour intermingled and sought to disentangle these and to reconstruct an historical stratification; whether fuller knowledge of Melanesia confirms or destroys this historical sequence is not perhaps of vast scientific import, but it was during this research that Rivers elaborated his theory of culture contact, and the interaction of cultures, which has been a most fruitful stimulus to anthropology. Though Rivers was here concerned with "what has happened," he incidentally threw much light on "why it has happened" by drawing attention to the clash of different types of culture with one another and the varying results. Therein lies the fundamental difference between his theory and that of the school of Graebner and Schmidt, which is founded on material culture and not on psychological data.

The papers entitled "Sociology and Psychology," "Freud's Concept of the Censorship," "The Primitive Conception of Death," do not deal primarily with the theories of diffusion or contact, but in these Rivers criticizes current views of psychology and sociology. While he completely rejects Levy Bruhl's pre-logical mentality he gives value to the law of participation and lays stress on need of understanding native categories, put forward by the French School, which he illustrates by the Melanesian conception of death. This conception of death, so different from our own, is also to be found in Africa. While among the Lotuko in the Sudan, the reviewer heard the death-wail and was told that a woman had died; on visiting the hut she was surprised to find the woman still living, though the death ceremonies had begun. The paper on "Trade, Warfare and Slavery" also deals with the right understanding of native categories.

The inclusion of the criticism of Freud's concept of the Censorship is of interest, as it serves to remind the reader that Rivers recognized the great importance of Freud's work, although the present volume is devoted to the support of a theory in actual opposition to Freud's.

It is difficult to account for the arrangement of chapters: the article on "Trade, Warfare and Slavery" might equally be included under the heading *Psychology* as that on the *Conception of Death*, while the *Sexual Relations and Marriage in Eddystone Island*, placed under *Psycho-Medical Studies*, is a straightforward piece of descriptive social anthropology, presumably taken direct from field notes; there is no date or record of previous publication. The two other papers under this heading could stand also under the title *Diffusion*. Some of the papers are dated, others though previously published bear no indication of date or place of publication. This is all the more to be regretted as one of the chief interests of this volume is to trace the process of Rivers' adoption of the theories of Prof. Elliot Smith and Mr Perry. With this in mind *The Ethnological Analysis of Culture*, Presidential Address to Section H, at the Portsmouth meeting (1911) of the British Association, is the first paper to be read. Here Rivers explained the complexity of Melanesian society by the blending of peoples, and sketched the history of his 'conversion' to this theory by means of the study of his own material, comprising very largely as it did the study of kinship systems. Those who take an extreme view of the importance of diffusion should note Rivers' words in this article: "Because I have insisted on the importance of ethnological analysis, that does not imply that I underrate the need for the psychological study of customs and institutions." "It is only by the combination of ethnological and psychological analysis that we shall make any real advance." Then comes a series of studies on culture contact, demonstrating the type of evidence that he considered important. Among these are *The Disappearance of Useful Arts* (1912), *The Contact of Peoples* (1913), in which the influence of the western culture of the present day on the higher civilizations of Asia, and on those of Africa, Melanesia, and America, is contrasted, and the principle formulated that the extent of the influence of one people upon another depends on the difference in the level of their culture. In the light of this principle the complexity of Australian culture is examined, and Prof. Elliot Smith's theory of megalithic culture put to the test.

Massage in Melanesia (1913) is a slight study of a practice which Rivers states he might previously have regarded as an example of the evolution of a therapeutic measure from a magical one, but here looks upon as the transmission of a therapeutic measure to a lower culture with degeneration in the direction of magic. *The Double Canoe* (1914) is a discussion on the origin and distribution of that vessel; he was unable to draw any definite conclusion but again points out the type of evidence necessary for a decision. *The Irrigation and Cultivation of Taro* (1914) is a more detailed study, and the practice is held to be carried by the Megalithic Culture. The description of *A Modern Megalithic Culture* (1915) is disappointingly slight, considering its importance to the theory. In this study Rivers, while apparently satisfied that the megalithic monuments of the world belong to one culture, seeks a solution to the problem of ascertaining which cultural elements would be carried on the wave of migration that spread these buildings, by the examination of customs in the New Hebrides. The danger of arguing in a circle, of which he showed himself fully aware in his *History of Melanesian Society*, is here disregarded. Nor unfortunately has he defined the term megalithic. Are all large dry stone buildings to be included, as for instance cyclopean walls, and such monumental structures as Tiryns?

The Distribution of Megalithic Civilization (1915) is a statement of Rivers' adherence to the view of Prof. Elliot Smith and Mr Perry that Egypt was the centre from which the diffusion of culture had arisen. Before that date Rivers had hesitated to accept this view, but was convinced by the similarities in the method of mummification of Egypt and Torres Straits demonstrated by Prof. Elliot Smith. But he still felt uncertain about the list of practices and beliefs carried from Egypt and starting about the eighth century B.C. that was then postulated by Prof. Elliot Smith. Rivers considered the distribution to be due to more than one migration. He adopted Mr Perry's theory that the motive of these migrations was the search for wealth, but added no fresh evidence in support of this idea. *Sun Cult and Megaliths* (1915) is a further contribution to the theory, drawn from the examination of secret societies in Oceania.

The Concept of Soul Substance in New Guinea and Melanesia (1920), another contribution based on psychological data, brought Rivers to form the surprising but not unwarranted conclusion that ancient beliefs are recorded with the greatest fidelity not near their old home but in isolated spots at great distance from it. However, far more detailed research, both from historical records at the centre and psychological investigation at the supposed points of diffusion, are needed before such a hypothesis could be accepted as a guiding plan. Here the reader may be referred to the article on *Convergence*, unfortunately undated. The possibility of convergence is not summarily dismissed, though it is never considered to be a final solution of the problem: "Convergence will be a useful concept in ethnology only if we recognize that the convergence may be historical or psychological, or, as will probably be found more frequently, dependent on a process which can only be fully understood when studied by the combination of the historical and psychological methods" (p. 150).

The reader should now turn to the article on *Circumcision, Incision and Subincision*, bearing in mind the editor's statement (pp. xvi–xvii): "But at the time (1915) the lectures included in the latter book [*Medicine, Magic, and Religion* (1924)] were being written, Dr Rivers was beginning to realize that he had pushed the theory of mixture of cultures further than the evidence warranted. In other words, he found that he was attempting to explain as the result of the blending of two elements in Melanesia practices that were introduced into the region from elsewhere in essentially the same complex form. As the facts relating to circumcision and incision were mainly responsible for convincing Dr Rivers that the hypotheses tentatively put forward in *The History of Melanesian Society* needed modification, the essay entitled 'Circumcision, Incision and Subincision' has been included in this volume." Taken in conjunction with the hint thrown out in the preface to *Medicine, Magic, and Religion* that the view of the blending of different cultures had "dominated his thoughts between 1911 and 1918, but from which he was gradually becoming emancipated during the last four years of his life," this essay becomes particularly interesting. It is greatly to be regretted that it is undated; part of it however was published in *Medicine, Magic, and Religion* (pp. 103–105), though this is not mentioned in the present volume. It is impossible briefly to weigh up the evidence and arguments of this highly suggestive essay; it must suffice to mention that Rivers regarded circumcision and subincision as wholly separate in origin and nature. No reference is given for the statement that incision "was practised in Egypt from the earliest predynastic times till the dynastic period, when it was supplemented by circumcision" (pp. 63–64), nor is there any available evidence published for it. Incision is portrayed on the figures in low relief on the celebrated Gerenuk palette. This is protodynastic; moreover the incised men are not Egyptians, for they are defeated enemies and have woolly or frizzly hair, so that they probably belonged to a mixed Hamito-Negroid stock (*Annals of Archaeology and Anthropology*, vol. VII, pp. 43–9)[1]. This misstatement is noted because so much importance is attached to the essay and circumcision is one of the practices that is supposed to have spread with the megalithic culture; it does not however alter the argument, which is chiefly concerned with subincision. This latter mutilation, so characteristic of Australian culture, was at one time believed to be restricted to that continent, but has now been shown to exist, not as a ceremonial but as a therapeutic practice, in Fiji, whence it has spread to Tonga. The discharge of blood is believed to be part of its beneficial action. The hypothesis suggested is that "some migrant people, who practised subincision, either as a therapeutic practice or a ceremonial rite, introduced it into Fiji and Australia, and that in the process of assimilation into the indigenous culture of the two places it has undergone such transformation as now gives it its wholly different purpose in the two places." It is then considered to be more likely that the migrants introduced this operation as "a remedy for disease, and that this has largely maintained its original purpose in Fiji, while in Australia it has taken on the special magico-religious purpose characteristic of the aboriginal Australian culture." The presence of similar beliefs regarding pathology in Eddystone Island, although no subincision operation is found, is considered to be a link in the chain. But it is thought that if such an operation as *tokalosi* (the Fijian form) had been introduced the surgical skill of the inhabitants would not have been adequate for them to ingraft the practice into their own culture. But why the Eddystone Islanders should be considered so inferior in skill to the Australians is not clear.

In this essay reasons are given why subincision should not be regarded as a development of circumcision (although it is suggested that the latter is itself derived from incision), and it is supposed that the former practice was carried by one cultural wave to Fiji, Eddystone, and Australia, with quite different results in each locality. No reasons for the single culture wave are brought forward, and it is difficult to see the necessity for adducing them, considering no difficulty is seen in accepting the relatively recent borrowing of the Tonga custom from Fiji. The home of subincision is nowhere stated, but as it is considered to be carried by migrants of a superior culture it may be assumed that either Egyptians themselves or peoples imbued with Egyptian culture are inferred. In actual fact there is no evidence that Egyptians knew of this operation. Further, the relationship between subincision and circumcision and incision is left obscure, and even if there were evidence to prove that incision was replaced by circumcision in Egypt no suggestions are made to account for the migrants carrying the discarded as well as the existent practice.

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[1] In *The Ancient Egyptians* (1923, p. 62) by Elliot Smith it is stated: "There is reason to suppose that the operation was performed at the age of puberty, as an initiation to manhood, and at first was not true circumcision, but what Dr Rivers calls 'incision,' *i.e.* merely slitting the foreskin and not amputating it." But Professor Elliot Smith does not bring forward any evidence for this supposition, in fact the next few lines are vague.

It seems that this essay hardly warrants the supposition that Rivers had come to regard the diffusion of one highly complex culture as a more useful explanation of human institutions than the theory of the mixture of cultures, although he had accepted the pan-Egyptian hypothesis (p. xvi). If however he had really come to this conclusion much if not all of his previous work would need to be discarded or re-written, and this refers especially to his work on social organization, which he himself considered his most important contribution to Ethnology.

Probably the Diffusion *v.* Evolution controversy has already done much to obscure the real value that has been gained by the rejection of the ultra simple evolutionary scheme, such as the conception that culture contact gives rise to new social forms, and the necessity for caution when considering any human institution lest by regarding only its present function we ascribe its origin to a corresponding single impulse. But however fascinating the study of history may be, and illuminating in elucidating the complexity of customs, the description of functions still remains the work of the ethnologist. The theory of diffusion leaves us with our main problem of human institutions unsolved. Should sufficient evidence ever be brought forward to establish it, with a single centre for the diffusion of all culture beyond a food-gathering stage—a state of society in which if I understand the theory aright, the family was the sole social institution and the arts and crafts were of the simplest type compatible with human existence—should this really have been the course of history our problem would be no easier. The spontaneous development of one culture in one place by one people from the simplest form to a highly organized political entity is assumed. It is surely a strange paradox that the very school that scorns the possibility of the evolution of culture in other places should hypothesize such an uninterrupted type of evolution in the Nile Valley, while the world-wide spread of this culture and its reception by savages, apparently persisting throughout the world in mental cold storage in the most primitive condition, is a hypothesis that seems to ignore all the psychological and economic needs of mankind.

Such wonders are surely unnecessary for the explanation of the great similarity that may be found beneath the diversity of human beliefs and behaviour. The great facts of life, procreation, birth, and death, with their accompaniments of immaturity, adolescence, maturity, and decay, have always been the same, and should be sufficient to form a common background. The same instincts must be aroused though varied environment may bring into prominence different combinations of sentiments. On the other hand geographical, hence economic environment, as well as culture contact, must always react on human institutions. But Man, as contrasted with other animals, has two great weaknesses—his long immaturity and the absence of a mating season. If he had not developed social institutions that protected the immature, and, while the sexual urge was never given a resting season, yet produced a form of society stable enough to prevent constant disruption, he would have disappeared long ago. It seems that the formation of incest barriers made possible the human family, as contrasted with the animal family which disrupts on the young attaining to maturity. The human family was then a social institution, not a natural phenomenon, while there is evidence that suggests the existence of social groups larger than the family as early as late palaeolithic times, in any case we know that religious beliefs and sanctions existed at this period long before any single unifying migration could have started around the world.

PUBLICATIONS RECENTLY RECEIVED

Ability. By VICTORIA HAZLITT. London: Methuen & Co. Pp. vi + 154. 6*s.* net.

Dr Hazlitt has written an interesting and significant book to the production of which a large amount of careful and painstaking research must have been devoted. In the first part of it she discusses generally the nature of ability, dealing both with "general capacity" and with "special abilities." She adopts the general view that intelligence is the organisation of mental processes for the purpose of solving problems, and urges that the "general factor" which it involves is "the measure of the degree to which experiences influence one another, or are confluent." She believes this "confluence" to be determined by heredity. There are a number of interesting remarks concerning "configuration," which is treated as "the unconscious process that is involved in the solution of a problem," and many attempts are made also to link up the view with biological and neurological work upon the functions of the cerebral cortex, and in particular with the work of Professor Elliot Smith and Sir Henry Head.

In the second part of the book an account is given of a prolonged investigation, by specially devised tests, of the abilities displayed by university students in Arts and Science Faculties respectively. Many of the tests are extremely interesting and should be more widely used.

The book is very readable. No doubt Dr Hazlitt would be the first to admit that both her results and her theories need further testing. In the meantime she has produced a suggestive and well thought out contribution to a difficult topic.

Psychological Studies. By THEODOR LIPPS, Second, revised and enlarged edition translated by HERBERT C. SANBORN. London: Baillière, Tindall and Cox. 1926. Pp. 333. 27*s.* net.

This is the second volume to appear in the *Psychological Classics* series under the general editorship of Professor Knight Dunlap. It should be extremely valuable to all serious students of psychology. The contributions of Lipps contained in this volume are *The Space of Visual Perception*, *The Nature of Musical Consonance and Dissonance*, and *The Law of Psychic Relativity and Weber's Law*. The translation is both good and very pleasant to read, and all persons concerned are to be warmly congratulated on their enterprise in the production of this volume.

Shell Shock and its Aftermath. By NORMAN FENTON. London: Henry Kimpton. 1926. Pp. 173. 12*s.* 6*d.* net.

This is an extremely interesting and valuable study of the incidence and method of treatment of neuroses and psychoneuroses as they occurred in the American Expeditionary Force in the late war. The author had exceptional opportunities of studying the cases actually during their acute stages, and also of following them up after an interval of six years. The psychological and social significance of his study is very great, and he presents all his results with excellent terseness and caution. In fact this is a book that should be read carefully by everybody who is interested in war neuroses, or in military organization in general; and it also contains a great deal of significant matter for the student of mental diseases in civilian life.

Brain Capacity and Intelligence. By Dr E. MORRIS MILLER. Monograph Series, No. 4. Australasian Association of Psychology and Philosophy. Sydney. 1926. (Macmillan & Co.) Pp. 79. 2*s.* 6*d.* net.

This carefully written monograph contains results of a comparison of a large number of "brain measurements" of Tasmanian and Victorian school boys, and an enquiry into the educational significance of those measurements. There are also a study of the head size and mental levels of Tasmanian prisoners, and a discussion of the use of measurements of the type given as a means of diagnosing mental defect.

The Mind in Disease. By M. P. LEAHY. Heinemann: London. 1926. Pp. 178. 6*s*. net.

This is a sensible and straightforward book. The author makes no absurd claims but he gives a number of useful illustrations of how "a hopeful mental outlook can do much to improve the patient's condition in disease." The only objection necessary to bring forward is that books of this sort may produce an impression that the utilization of "the mind in disease" is a much simpler matter than it really is. The author writes clearly and without technicality.

Aphasia. By S. A. KINNIER WILSON. London. 1926. Kegan Paul Ltd. Pp. 108. 2*s*. 6*d*. net.

Types of Mind and Body. By E. MILLER. London. 1926. Kegan Paul Ltd. Pp. 132. 2*s*. 6*d*.

Man not a Machine. By EUGENIO RIGNARO. London. 1926. Kegan Paul Ltd. Pp. 75. 2*s*. 6*d*. net.

These three little books all belong to the "Psyche Miniature Series." Each is a good presentation of its subject-matter, clear, and suitable for the general reader. Dr Kinnier Wilson deals with aphasia first from an anatomical, then from a physiological, and finally from a psychological point of view, and attempts to give to each standpoint its due weight. He passes on to consider some of the many classifications of aphasic disorders, presents a number of interesting clinical observations, and concludes with a section on methods of treatment.

Dr Miller considers the correlation of physiological and psychological characteristics, a study which has received considerable impetus from the recent work on the endocrine glands and from the classifications put forward by Dr Ernst Kretschmer. His account is both attractive and stimulating, and will serve to emphasize how large is the number of unsolved problems both in physiology and in psychology in this field.

Professor Rignano presents a brief account of his well-known views of the general nature of biological development and writes with all his accustomed persuasiveness and enthusiasm.

Each of these books is well designed to introduce to the notice of the student a fascinating field of study which he will desire to explore further.

The Realm of Mind. By FREDERICK J. E. WOODBRIDGE. New York: Columbia University Press. 1926. Pp. viii + 141. $ 1.75.

Dr Woodbridge calls this "an essay in metaphysics," and states his own thesis as follows: "mind as a logical structure is antecedent to thinking, and...our thinking as individuals is a bodily activity congruent with that structure." The essay is most attractively written, and contains a number of acute psychological observations as well as some more recondite philosophical argument.

Philosophy of the Recent Past. By R. B. PERRY. London: Charles Scribner's Sons. 1927. Pp. viii + 226. 10*s*. 6*d*. net.

The movements dealt with are: naturalism, materialism and positivism, spiritualism and idealism: vitalism, voluntarism and pragmatism, and the revival of realism. A conclusion sums up the philosophic tendencies of the immediate present. There is a selected bibliography.

Mind and its Place in Nature. By DURANT DRAKE. New York: The Macmillan Company. 1925. Pp. xv + 259. 8*s*. 6*d*. net.

Although this book is written mainly to expound a radically realistic view of the ultimate nature of mind and matter, it contains not a little that may interest the more theoretically minded psychologist. It has two parts: one dealing with nature and the other with mind, and there is a brief conclusion in which the two are brought together.

An Adventure in Moral Philosophy. By WARNER FITE. London: Methuen & Co. 1926. Pp. x + 276. 10*s.* net.

Somehow the title of this book rather suggests a kind of excitement and risk which the book itself fails to maintain. The volume is a genial, over long essay on morality addressed to the "cultivated man" and attempting to show the value of the workings of self-consciousness throughout human life.

La Perception Visuelle. Par RENÉE DÉJEAN. Paris: Félix Alcan. 1926. Pp. 168. 35 Fr.

This book contains a discussion of the problems of vision based upon existing records of experiment. In the first part the visual perception of distance is treated and its relations to other sensory reactions. Eye movements, binocular fusion, etc. are discussed. The second part consists of a consideration of objective and physiological conditions affecting visual spatial perception.

Scientific Humanism. By LOTHROP STODDARD. London: Charles Scribner's Sons. 1926. Pp. 1 + 177. 7*s.* 6*d.* net.

Dr Stoddard has here written, in an enthusiastic vein, a survey of many contemporary social developments particularly as they appear in America. He believes that there may be hope for the world yet if only "the common man will hearken to the voice of reason rather than to the tocsin of passionate emotion."

The Law Within. By Sir BAMPFYLDE FULLER. London: Kegan Paul, Trench, Trubner & Co. Ltd. 1926. Pp. viii + 295. 12*s.* 6*d.* net.

The book is really a general treatise on psychology. It deals with (1) nervous origins, (2) motives, (3) sensation and thought, (4) behaviour and (5) experience and education. The treatment cannot be described as in any way exciting or as particularly original. Nevertheless the author has much that is interesting to say and he says it gracefully.

The Editor regrets an error which appeared in the brief notice of *Modern Psychology and Education* (This Journal, XVII, 260). This book was written by *M. Sturt* and E. C. Oakden, *with an introduction* by T. Raymont.

COMMITTEE FOR RESEARCH IN EDUCATION

With a view to facilitating the general co-ordination of educational research, the Committee has made its annual enquiry as to researches in progress (or planned) at the various University Psychological Laboratories and Educational Departments.

The following is a summary of the replies received:

RESEARCHES IN PROGRESS OR PLANNED, DECEMBER, 1926.

BEDFORD COLLEGE FOR WOMEN (UNIVERSITY OF LONDON).

1. Study of the Special Abilities involved in following the curriculum of the Faculty of Arts and the Faculty of Science respectively. (This work being continued by Dr. V. Hazlitt.)
2. A Study of Eidetic Imagery. (Miss J. A. Jenkin.)

UNIVERSITY OF LONDON, UNIVERSITY COLLEGE.

1. Psychology of Error.
2. Perception of Time.
3. Development of Concepts.
4. Application of Rules and Examples.
5. Motor Dexterity.
6. Bi-lingual Education.
7. Engineering Ability.
8. Tests of Mathematical Aptitude.
9. Perception of Children.
10. Reasoning.
11. The Power of Adaptability to New Situations.
12. Individual Differences of Ability.
13. Emotion.
14. Conation.
15. Mental Oscillation.
16. Fluctuations in Mental Output.
17. Effect of Practice on Mental Testing.
18. Tests of Primitive Races.

UNIVERSITY OF BRISTOL (EDUCATION DEPT.).

1. The influence of practice on some component tests of Intelligence Examinations.
2. Transference of Mental Fatigue.
3. Factors in Temperament and Ability in Teaching.

UNIVERSITY OF BIRMINGHAM (EDUCATION DEPT.).

1. Tests of Higher Mental Processes. (Prof. C. W. Valentine.)
2. The Psychology of Early Childhood. (Prof. C. W. Valentine.)
3. The Reliability of Intuitive Judgments. (Prof. C. W. Valentine.)
4. Vocational Selection among Secondary School Pupils. (Prof. C. W. Valentine.) (In collaboration with Miss F. M. Richie.)

5. School Children's Preferences in Poetry. (A. E. CHAPMAN, M.A.) (In collaboration with the PROFESSOR OF EDUCATION.)

6. The Idea of Beauty: Its development in the Visual Arts. (Mrs D. M. LALL, B.A.)

7. The Development of Humour in Children. (Mrs L. E. SIMPSON, B.A.)

GEORGE COMBE PSYCHOLOGICAL LABORATORY (UNIVERSITY OF EDINBURGH).

1. The correlation of subjects in the Intermediate curriculum. (Continued.)
2. An investigation of the causes of backwardness in school. (Continued.)
3. An investigation into character and temperament tests. (Continued.)
4. The devising of a Scheme of Mental Tests for the Deaf.
5. A Study of the Vocabularies of deaf children.

UNIVERSITY OF GLASGOW (EDUCATION DEPT.).

1. Two Research Students (for Ph.D.), one working at the problem of the backward child in small country schools, the other at the problem of estimating fitness for the Secondary School (involving a comparison of Examinations and Tests, among other things).

2. An Educational Clinic has just been started as part of the joint work of the Education and Psychology Departments of the University—for the purpose of exploring the problem of the difficult child—whether the difficulty be one of behaviour or learning—in the ordinary school. The problem of defectives or delinquents is not being touched, the inquiry being limited to the peculiarities of otherwise normal children. At present working at a technique, but intention is to make the Clinic a centre of research in regard to the work of the ordinary school. (Under joint direction of Dr. WM. BOYD, Dr THOULESS, and Dr SHEPHERD DAWSON.)

UNIVERSITY OF GLASGOW (PSYCHOLOGY DEPT.).

1. An Investigation on the Results of different Methods of Scoring in the Formboard. (Mr SMITH.)

2. The Rhythm of Speech. (Miss MACALISTER.)

UNIVERSITY OF LEEDS.

1. Visual Adaptation. (E. F. BENNETT.)
2. Artistic Appreciation and the Teaching of Art. (W. F. CREWDSON.)
3. Perception and the Learning Process. (C. F. EXLEY.)
4. Musical Ability in School Children. (H. LOWERY.)
5. Imagery in Literature. (J. E. MASON.)

UNIVERSITY OF MANCHESTER.

A year's leave of absence was granted, to permit acceptance of an appointment to the Faculty of the State University of Iowa, for the purpose of investigating recent developments in Physical Education in the United States, with special reference to University work.

Advantage was taken of the opportunities that offered of visiting other Universities and large cities on both East and West Coasts and becoming acquainted with movements for the promotion of personal and community welfare. (Miss W. S. CLARKE.)

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

1. The Distribution of so-called Mechanical Ability among Secondary School-boys and its correlation with intelligence, hobbies, interests, and out-of-school environment.

UNIVERSITY OF SHEFFIELD (EDUCATION DEPT.).

1. A Study of Motor Ability.
2. Scholastic Ability in relation to size of Family.
3. A Study in Children's Vocabularies.
4. Interpretation of Correlation Coefficients.
5. Development of Reasoning in Children.
6. Problems of secondary school education in the light of the principles of group psychology.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

February 12, 1927.	An Attempt to Internationalize Intelligence Tests, by Stuart C. Dodd. The Influence of Conation on Cognition, by E. H. Wild.
March 7, 1927.	La première année de l'enfant, by Jean Piaget.

SECTIONAL MEETINGS.

AESTHETICS SECTION.

January 14, 1927.	Recent Experiments in their bearing upon Aesthetics, by Ll. Wynn Jones.
February 11, 1927.	Beauty in Art and Nature, by L. Arnaud Reid.

EDUCATION SECTION.

February 14, 1927.	The Philosophical Basis of the Teaching of Literary Appreciation, by Cloudesley Brereton.

INDUSTRIAL SECTION.

February 27, 1927.	The Group Mind in Industry, by C. Delisle Burns.

MEDICAL SECTION.

January 26, 1927.	An Attempt to investigate and treat Cases of Psychoneuroses and Psychoses at an Out-Patient Clinic, by Thomas S. Good.
February 23, 1927.	Observation on the Formation and Function of the Super-Ego in Normal and Abnormal Mental States, by Sylvia M. Payne.
March 23, 1927.	Criminal Tendencies in Normal Children, by Mrs Melanie Klein.

PROCEEDINGS OF BRITISH PSYCHOLOGICAL SOCIETY
SCOTTISH BRANCH

February 13, 1926.	In George Combe Psychological Laboratory, University, Edinburgh: "The Present Position regarding the question of General Ability," by Professor Godfrey H. Thomson. "The Use of Intelligence Tests in Control Examinations," by George Mowat.
March 6, 1926.	In George Combe Psychological Laboratory, University, Edinburgh: "The Special Schools as a Field for Psychological Research," by D. Kennedy-Fraser.

November 13, 1926,	In Psychology Department, University, Glasgow: "The Physics of the Psycho-Galvanic Reflex," by R. H. Thouless.
December 4, 1926.	In George Combe Psychological Laboratory: "Some Assumptions in Psycho-Analysis," by W. H. Bryce. "Some Problems of the Experimental Method in Psychology," by C. A. Mace. "The Effect of Encephalitis Lethargica on the Intelligence of Children," by Shepherd Dawson and C. J. M. Conn.
February 5, 1927.	In Psychology Department, University, Glasgow: "Experimental Investigations into the Mental Resemblances of Twins," by Norman T. Walker. "The Causes of Backwardness—some Preliminary Results," by Mary M. McTaggart.
March 5, 1927.	In George Combe Psychological Laboratory, University, Edinburgh: "The Psychology of Public Worship," by W. P. Paterson. "The Rational Factors in Religion," by W. M. R. Rusk. "A Case of Religious Phantasy," by W. R. D. Fairbairn.

BRITISH PSYCHOLOGICAL SOCIETY

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the Meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of members.

12. Every Candidate for election to the Society shall be recommended by at least two members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from Membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to sub-divide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall

immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, Chairman and Secretary of each of the Sections and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two ordinary members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of members to serve on the Council may be suggested by the Council or by any member by sending the proposed names to the secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council or by ordinary members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent members, to whom the resolutions adopted shall be forwarded.

29. The first meeting of each Special Section shall be convened by the General Secretary of the Society. At this meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall

be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or failing him a Vice-President, shall whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all members of the Society notices of each meeting at least seven days beforehand, and of the business to be transacted at the meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty members; at least fourteen days' notice of such meeting shall be sent to each member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any meeting. The Council or a Sectional Committee shall however have the power of closing any meeting or meetings to visitors, provided notice to this effect be given on the notice convening such meeting or meetings. The names of visitors together with the names of members introducing them shall be entered in a book provided for that purpose. Any member of the Society attending a meeting of a section to which he does not belong must be introduced by a member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of ordinary members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as ordinary members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.